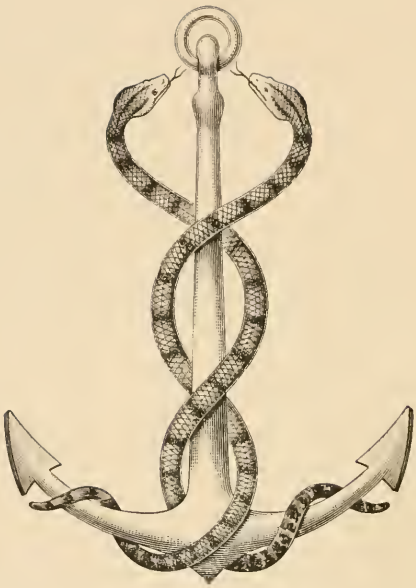
NATURAL SCIENCE



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NATURAL SCIENCE

# Natural Science

A Monthly Review of Scientific Progress

January 1899

## NOTES AND COMMENTS.

### Pure Science.

The debt of the world to pure science was the subject of Prof. J. J. Stevenson's (retiring President's) address to the New York Academy of Sciences on February 28, 1898, and the lecture is now available in the last volume of the *Annals* (xi. (1898) pp. 177-192). The thesis is an interesting one—"That the foundation of industrial advance was laid by workers in pure science, for the most part ignorant of utility and caring little about it," and the evidence should give pause to those impatient utilitarian spirits who would weigh all scientific work on the balance of "practical results." From the history of astronomy and geology, physics and chemistry, botany and zoology, examples are taken which show that what might have seemed out-of-the-way investigations—prompted by the curious or contemplative spirit—have had the most unlooked-for and far-reaching results in practice. It is not, indeed, to be forgotten that the converse thesis is also true,—that practical lore has in hundreds of cases reacted upon theory. The investigator prepares for the inventor, but the inventor in turn stimulates the investigator. Yet is there not a danger lest even this eloquent address may suggest that "pure science" requires an apology—a heresy which Bacon long ago confuted in his distinction between experiments which are "lucifera" and those which are "fructifera"? Is there not reason to recall his words: "Just as the vision of light itself is something more excellent and beautiful than its manifold uses, so without doubt the contemplation of things as they are, without superstition or imposture, without error or confusion, is in itself a nobler thing than the whole harvest of inventions"?

### A Contribution to the Biology of Cock-Fighting.

E. Bordage, who has previously made some interesting observations on various cases of regeneration, has recently turned his scientific eye upon cock-fighting. It is one of the pastimes of Réunion, where the pugnacity of the cocks is such that the premaxillae and the anterior parts of the mandibles are sometimes torn away in the fury of combat.

In two or three months they are regenerated, alike as regards bony and horny tissue. The regeneration of the mandibular portion proceeds more slowly than that of the upper parts. The author entitles his paper "Cas de régénération du bec des oiseaux expliqué par la loi de Lessona" (*C. R. Soc. Biol.*, July 1898), but as Lessona's law is hardly as familiar as Boyle's we may be allowed to state it. In 1868, the illustrious Italian naturalist formulated the conclusion that in animals the parts which are capable of regeneration are those which run most risk of mutilation. This conclusion has been restated by Darwin and by Weismann (whose name the author spells in the usual French fashion) in the thesis that regeneration is an adaptive phenomenon. Weismann found some difficulty in regard to a stork which repaired the terminal half of its mandibles. But Bordage points out that male storks fight furiously. After this who can deny that cock-fighting has its utility?

## A Delicate Operation.

In a short paper entitled "Embryons sans noyau maternel" (*C. R. Ac. Sci.* cxxvii. (1898), pp. 528-531) Professor Yves Delage describes a remarkable experiment. He divided the egg of a sea-urchin under the microscope into two parts, one containing the nucleus and the centrosome, the other simply cytoplasmic. Beside them he placed an intact ovum, and then let spermatozoa in. All the three objects showed equal "sexual attraction," all were fertilised, and all segmented, the intact ovum most rapidly, the nucleated fragment more slowly, the non-nucleated fragment more slowly still. In one case, the development proceeded for three days; the intact ovum had become a typical gastrula, the nucleated fragment a smaller gastrula, and the non-nucleated fragment also a gastrula, but with very much reduced cavities, "presque virtuelles." All the cells showed nuclei. The experimenter concludes that fertilisation and some measure of development may occur in a fragment of ovum without nucleus or ovocentre. It is probably by these indirect paths that we shall eventually discover the real nature of fertilisation. According to Delage, two things must be distinguished—(*a*) the stimulus given to the ovum by a specially energetic kinoplasm brought in by the spermatozoon, perhaps in its centrosome; and (*b*) the mingling of heritable characteristics, or amphimixis. It must, of course, be observed that while the fertilised cytoplasmic fragment in Delage's clever experiment segmented, it did not really develop.

## Birds and their Surroundings.

The British Museum (Natural History) has recently placed on exhibition a model by the American ornithologist, Mr. A. H. Thayer,

which shows in a striking manner the value of coloration harmonising with the surroundings. This model consists of a box lined with light gray felt, but open at the top and front. In the middle of the box are two rough figures of birds, each identical in size, and sitting upon a perch. One is covered with the same gray felt as lines the box, and the other is darker in shade above than the felt and lighter than the felt below. A spectator standing close to the box sees both birds clearly, but three or four feet away the darker bird is almost invisible, and at six or eight feet entirely disappears from sight. The reason for this is that the lighter under-colour of the darker bird counteracts the shadow thrown by the top light, and makes the bird appear an uniform colour at a little distance, while in the case of the bird of the same colour as the surroundings a shadow throws it into strong relief. This exhibit, which strikingly demonstrates colour values, will be found in the small gallery leading from the central hall to the British birds.

## The Skin and Fins of Ichthyosaurus.

Dr. Eberhard Fraas, to whom we are indebted for our first information concerning the outward form of the body in the extinct marine reptile *Ichthyosaurus*, has just published a description of another specimen from the Upper Lias of Wurtemberg, now in the National Museum at Buda Pesth (*Földtani Közlöny*, 1898, vol. xxviii. pp. 169-173, pl. ii.). It is now absolutely certain that this dolphin-shaped reptile had a small triangular fin on its back, and that the vertebral column turned downwards into the lower lobe of the laterally-compressed forked tail. It seems probable, however, that the newt-like crest of skin supposed to extend along the back behind the dorsal fin, was a deceptive appearance in the first specimen described. Four examples of *Ichthyosaurus*, showing the outline of the body, are now known from the Upper Lias of Wurtemberg—the first in the Stuttgart Museum, the second in a private collection at Brussels, the third at Buda Pesth, and the fourth probably destined for the Museum of the University of Tübingen. There is also a well-preserved detached tail from the Bavarian Lithographic Stone in the Palaeontological Museum, Munich. Thus rapidly has our knowledge of the subject progressed since Dr. Fraas first communicated his original drawing of the Stuttgart specimen to *Natural Science* in September 1892 (vol. i. p. 515).

## Books and the Customs.

In a note on the dates of publication of Temminck and Laugier's "Planches coloriées," that wonderful collection of illustrations of birds first issued by Buffon, which has just appeared in the *Ibis*, Mr. Davies Sherborn calls attention to a publisher's notice of much interest.

Quoy, who reviewed the parts as they appeared in the *Bulletin des Seiences Naturelles*, says in one of his notices that the authors had abstained from giving any text to the first twenty parts for fear of increasing the price of the book, and above all from their delicacy of writing after the illustrious Buffon. They were, however, compelled to do so because the foreign Customs rejected a collection of plates without text, considering them as the product of the arts of France rather than as a scientific work destined to be circulated all over Europe. This was in 1824. Times have changed since then, but we can recall an instance of stupidity on the part of British Customs officials even within the past decade, connected with a book on "Greek Vases."

## A Thorny Subject.

Mr. C. E. Beecher has published an interesting study on the "Origin of Spines" (*Am. Journ. Sci.* 1898, vi. pp. 1-20, 125-136, 249-268, 329-359, 73 figs.). He points out that the suggestion of protective function, so freely made, is not always valid; but although the elimination value of spines and related structures may be slight, yet the spinose condition itself can, he says, be shown to be of much importance. "It represents a stage of evolution, a degree of differentiation in the organism, a ratio of its adaptability to the environment, a result of selective forces, and a measure of vital power."

Mr. Beecher emphasizes the fact that spines or equivalent structures are rarely present in young forms, but appear during the process of growth, and he seeks to distinguish two types of spine-development. One type, well exemplified in the Barberry, shows that spines may develop from the degeneration of organs, as in plants from leaves, in animals from appendages, *e.g.* the spurs of the python, and so on. In the other, the ornamental type, well seen in the shell of *Spondylus*, the spines do not arise from a check to growth, but from an excessive development of functional parts. Further, just as in ontogeny spines of both types develop late, so phylogeny proves that spinose or ornamented forms have behind them a long history of simple unadorned ancestors. This is well seen in *Spondylus* itself, which is descended from forms with simple smooth shells; in many Brachiopods, which became spinose just before extinction; in the history of the whole phylum of Echinoderma, whose members have become increasingly spinose in geological time. Phylogenetic studies of this kind show also that the "maximum of generic, family, and ordinal differentiation is found at an early period, while the greatest specific differentiation occurs at a later period." That is, variation first affects physiological and internal structures, while later the changes are mainly physical and peripheral. In other words, in any developing group "the more important physiological and structural variations are the first to be subjected to heredity and natural selection, which tend to fix or hold

them in check. Features of less functional importance, as peripheral characters, are the last to be controlled, and therefore present the greatest diversity, while in this diversity spinosity is the limit of progress." As to the causation of the two types of spines, Mr. Beecher refers to the "general laws of organic change,"—that is, the stimulus or restraint of the environment, and the energy or deficiency of growth force. Of these four causes two tend to produce spines like those of *Berberis*, two to produce the ornamental type as in *Spondylus*. As no one of these causes is simple, and the action of each may be assisted by secondary causes such as sexual selection, mimetic influences, and so on, there are in all eleven causes of spine production, all of which are discussed in detail with illustrative examples.

The conclusions reached are, as indicated above, that on the one hand spinosity represents the limit of morphological variation, and on the other the decline or paracme of vitality. That is to say, alike in ontogeny and in phylogeny spines increase until maturity, and there is then in old age an "extravagant differentiation followed by a decline of spinous growth, and ending in extreme senility with their total absence." In other words, the great development of spinose organisms in a group represents the beginning of the decadence of the group.

## Philosophy of Evolution.

We cannot but welcome a paper of this type, more especially when, as in Mr. Beecher's case, the evidence brought forward is largely palaeontological. No one who has toiled through pages of controversy as to the probable action of natural selection in hypothetical cases, can fail to appreciate a genuine attempt to discover what natural selection has actually done in the past. But while recognising to the full the value and excellence of Mr. Beecher's work, it is difficult to avoid criticism of the frequent want of precision and clearness in his use of words, especially in the treatment of the philosophical aspects of the subject. The habit must, we think, lead sooner or later to much confusion of thought. For example, is any good end attained by the introduction of sentences like the following into a discussion of the question as to whether variation is limited or unlimited: "As far as can be seen, the limitations of the forms of species of animals and of plants end only with the aggregate number of possibilities within the functional scope of the organism. Beyond in either direction is death, and a passage from the organic to the inorganic"? The difficulty of comprehending this statement is in no way diminished by finding that the author seems to mean that variation is definite and orderly. Again, the author states that certain types of spines have been produced by the "law of repetition," which is "similar to induction in electrical physics, or to the force or stimulus of example in human conduct." Is not this merely biological mysticism? We also hear much of the "forces of sexual

selection, environment, and growth," as if all acted after a precisely similar fashion. Horns, we are told, may arise as the direct result of the action of the environment, but if they are confined to one sex it is "evidently a case of sexual selection." We do not suppose that Mr. Beecher regards this "force" as a kind of "Red Queen," whose fiat "Off with his head," or her horns, can be relied upon to assist the other "forces and laws" when they get into difficulties, but it is surely unfortunate that his language should lend itself so readily to such misapprehension.

## Eyes in Bivalve Mollusca.

THE Acephala, or headless Mollusca, as the Pelecypoda have been termed, whilst possessing eyes in the normal position near the mouth when in the larval condition, have hitherto been supposed to be quite devoid of such organs when adult. Some of them, it is true, have developed organs of vision elsewhere, for instance, in the mantle-margins (*Pecten*, etc.), or at the extremities of the siphons (*Cardium*), but these have no connection with true cephalic eyes.

Dr. Paul Pelseneer has, however, recently discovered the existence of true cephalic eyes in certain groups (*C. R. Ac. Sci.* cxxvii. (1898), pp. 735 and 736). Since his account is short we give a full translation :—

> There exists in adult Lamellibranchs a pair of distinct and well-formed cephalic eyes. They consist of pits with pigmented walls filled by a cuticular lens, and they are thus intermediate in their structure between those of *Trochus* and *Patella*.
>
> These organs appear to be peculiar to most of the Mytilidae (*Mytilus*, *Lithodomus*, *Modiolaria*) and to the allied genus of *Avicula* proper (exclusive of *Meleagrina*).
>
> They occur both in the larva and the adult, but only do not make their appearance in the former (*Mytilus*) after the formation of the first branchial filaments.
>
> They are situated at the base and on the axial face of the first filament of the internal branchial lamella, and are innervated from the cerebral centre. In the larva they are situated *outside* the posterior margin of the velum.
>
> They appear to be homologous to the larval eyes of Chitons, which are also outside the velum and at the sides of the cephalic region, but they are not homologous to the cephalic eyes of Gasteropods, which arise *within* the velar area.

## Those Cretaceous Gryphaeas.

THE extreme variability of the Gryphaeas has always rendered them difficult of study, and in no place has this difficulty been more fully understood than in the Texan Region. It is therefore with considerable satisfaction that we call attention to the 151*st Bulletin of the United States Geological Survey*, in which Messrs. R. T. Hill and T. Wayland Vaughan have attacked the Lower Cretaceous Gryphaeas of the Texan Region and evolved some order out of chaos. These mollusca

occur in countless thousands, form extensive masses of indurated strata, the outcrop of which can be traced for many miles; they are sometimes used for road metal, or collected and burned for lime. They have been named and renamed, until their specific identity is hidden under a mountain of literature, and this mountain Messrs. Hill and Vaughan have at last removed. The authors find that certain forms of these Ostreidae possess very distinct specific characters, have definite geological horizons, and are of the greatest value in stratigraphic work. And this conclusion seems to be quite an usual one when people take the trouble to collect a sufficient number of individuals of any species, and do not content themselves by describing a single specimen or every minute variation as of specific value.

The Gryphaeas of the Texan Region are now reduced to six species, and of these six the authors give no less than 231 figures, thus affording an excellent opportunity for every one to see the variability among the species. The figures are excellent, and the paper may be recommended as a specimen of what is wanted in palaeontology in this country—namely, a careful and critical study of important types rather than mere descriptions of a jumble of more or less fragmentary and doubtful specimens.

## The San José Scale.

This insect, the source of considerable loss and trouble in the United States, has apparently been localised as an inhabitant of Japan. At least Mr. Cockerell, Mr. Alexander Craw, and Mr. F. M. Webster have by careful search found it several times on trees received direct from that land. The latter of these authors has written a short paper in the *Canadian Entomologist* for July, in which he asks that a competent entomologist may now be sent to Japan to locate it, and to find the natural enemies of the insect so as to import them into America. As Mr. Webster points out, the expenses connected with such an investigation would be a mere nothing compared with the amount spent in trying to exterminate the scale in those localities in the United States where it has obtained a foothold.

## A Warlike Entomostracan and a Decrepit Trilobite.

From the Chemung group of New York—that is, from strata of late Devonian age—Mr. John M. Clarke, the well-known palaeontologist, describes and figures a new Phyllocarid crustacean (*15th Ann. Rep. State Geologist, N.Y.*). He names it the bristling shrimp horrent with javelins (*Pephricaris horripilata*). Each valve is fringed with some two dozen spikes, which make a truly imposing show. There is, however, nothing unusual in the development of striking armature upon crustaceans and their allies. Witness, for example, the headpiece of *Hemiaspis horridus*, Woodward, the tailpiece in various species of the

trilobite genus *Cheirurus*, or the carapace of the modern *Lithodes arctica*. Among the terrestrial Isopoda, usually so smooth, *Cubaris aculeatus*, Budde Lund, breaks out into spikes all over its body. Sometimes the soft abdomen of a spider becomes a regular fortress of obdurate angles. The new Phyllocarid would look rather less formidable were its valves closed, in what one may suppose to have been their normal position. External pressure may account for their being spread so widely open. A sign and result of this pressure is probably to be seen in the strong diagonal groove which each valve in the fossil exhibits.

From the same group Mr. Clarke is now able to re-describe another important crustacean, if a trilobite be a crustacean. In 1888 he founded the species *Bronteus senescens* on "a very imperfect fragment of a pygidium." Now, he has had at command "two essentially entire specimens." As so often with trilobites, the entirety refers only to the dorsal view, but this enables Mr. Clarke to enter upon an interesting discussion of the structural variations within the genus *Bronteus*, as compared with the times of appearance of its several species. *B. senescens* is said to be noteworthy, not alone for the rarity of all trilobites at the horizon indicated, but as in all probability the latest representative of the genus. The specific name, therefore, must be understood to refer rather to the genus than to the species itself as "growing old." For the species is regarded by Mr. Clarke as "a survival, with appropriate time modifications, of the proper expression of the genus." Evidently the representative of *Bronteus* was doing its best to march with the times, but there must have been some revolutionary spirit abroad ruthlessly bent on ending the dynasty of the Trilobites. The Phyllocarids are still with us.

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## More North American Fresh-Water Copepods.

We have lately received two further instalments of the *Bulletin* of the Illinois State Laboratory of Natural History (vol. v. 1898), a publication to which the attention of our readers has been called on several previous occasions. The first (pp. 225-270) contains a paper by F. W. Schacht upon the comparatively rare fresh-water Copepods belonging to the genera *Osphrantieum*, *Limnocalanus*, and *Epischura*. In a notice of a previous paper by the same author on the closely allied but much commoner genus *Diaptomus* (*Natural Science*, xii. p. 300), it was pointed out that America possesses, so far as that genus is concerned, a very characteristic fauna—not a single one of its species, for instance, being known on this side of the Atlantic. It appears that the same phenomenon is almost equally true of the remainder of the forms included in the family *Centropagidae*, now under consideration, for of the five American species dealt with, only one, *Limnocalanus macrurus*, Sars, is known to occur outside North America. In this case the animal is capable of living in both fresh and salt water, so

that the Atlantic probably presents no barrier to its passage from Europe to America, and *vice versa*.

The paper gives keys and descriptions of the very peculiar and interesting forms coming within its scope, but unfortunately it is not accompanied by any figures. It may be perfectly true, as stated, that "the species treated may be identified by figures already published," but this information is more tantalising than useful.

## The Biology of the Victorian Era.

THAT the Liverpool Biological Society is as vigorous as ever is evident from the twelfth volume of its Proceedings and Transactions, which is full of interesting material. It begins with an inaugural address by Mr. Isaac C. Thompson on "Advances in Biological Science during the Victorian Era." This was a large order for one lecture, and Mr. Thompson's courage cannot be doubted. The result is a very delightful retrospect, though it lacks that completeness of outlook which we find, for instance, in Huxley's masterly sketch of an even wider subject—the Science of the Victorian Era. The bulk of Mr. Thompson's address is devoted, as is just, to evolution doctrine, and Darwin's work fills up so much of the picture that many great achievements have been crowded out. We miss, for instance, any recognition of such distinctive advances as the study of the cell, the conception of germinal continuity, the "protoplasmic movement" in physiology, the beginnings of physiological embryology, and many more. We sometimes wonder if there is not a hint of megalomania in much of our talk about "evolution," when even the foundations of the doctrine are still so far from secure. But the picture given in this address is so pleasant and vivid that it seems ungracious to complain that there are not more figures in it. There are visible figures, too—an interesting innovation—for the paper is illustrated by photographs of Charles Darwin, Erasmus Darwin, Cuvier, Edward Forbes, Wyville Thomson, Huxley, Pasteur, Spencer, Haeckel, Wallace, Weismann, Lister, and Herdman.

## A Half-Century of Evolution.

As the wonderful century draws near its end we shall be deluged with retrospects, and truly these arbitrary time-boundaries may have their use in stimulating a feeling for history. It is to be hoped, however, that the note of jubilation which has already been loudly sounded by some will be softened by criticism, and that more will follow Mr. Wallace in recording the century's failures alongside of its successes. In his address to the section of zoology on the occasion of the fiftieth anniversary of the American Association for the Advancement of Science (August 1898), Prof. Alpheus S. Packard seems to have sought the *via media*, but we are not sure that he has always kept to

it. The first part of the address is entitled "A Half-Century of Evolution." He says that "a reasonable and generally accepted solution has been reached" of the problem "How did living beings originate?" "the evolution doctrine is based on the inductive method," though "biology is not an exact science"; "every department of intellectual work and thought has been rejuvenated and rehabilitated by the employment of the modern scientific method," and so on. Yet after this optimism, as it seems to us, he goes on to give a luminous sketch of the present conflict of opinions in regard to the very fundamentals of evolution doctrine, and he sums up: "With all these theories before us, these currents and counter-criticisms in evolutional thought bearing us rapidly along, at times perhaps carrying us somewhat out of our depth, the conclusion of the whole matter is that in the present state of zoology it will be wise to suspend our judgment on many theoretical matters, to wait for more light and to confine our attention meanwhile to the observation and registration of facts, to careful experiments, and to repeated tests of mere theoretical assumptions." This may be very sound advice, but it is rather a cold douche after the warmth of congratulation in which the author elsewhere indulges.

Prof. Packard seems to us to make a mistake in his historical summary, when he says "a third school or sect has arisen under the leadership of Weismann, who advocates what is in its essence apparently a revival of the exploded preformation, encasement, or 'evolution' theory of Swammerdam, Bonnet, and Haller, as opposed to the epigenetic evolutionism of Harvey, Wolff, Baer, and the majority of modern embryologists." Our reading of the history would lead us rather to say that Weismann differs as thoroughly from Bonnet as Hertwig from Harvey. The modern epigenesis differs *toto coelo* from that of the ancients. Are Prof. Whitman's essays not read in America?

## Is Telegony a Mare's Nest?

FOR some years Professor Cossar Ewart has been making experiments which bear upon the vexed question of telegony, or the supposed influence of a previous impregnation upon subsequent offspring. As may be remembered, he started with the colt Romulus, the offspring of a Rum pony mare (Mulatto) by a Burchell zebra stallion (Matopo). Romulus has markings quite different from those of his sire, and rather resembling the Somaliland zebra. In 1897, Mulatto had a grey colt foal to a grey Arab stallion, and this foal showed at first some subtle markings slightly suggestive of zebra-influence. More careful examination of the skin after death showed that the hints of stripes were merely due to dispositions of the hair. In short, the experiment furnished no evidence in support of the hypothesis of telegony. In another case, a skewbald pony mare had, by the zebra, a hybrid which was fairly well marked, and by a bay Shetland pony a second foal

which almost exactly resembled its dam. In other cases similar negative results were obtained, any remarkable peculiarities that occurred being interpretable rather as reversions provoked by hybridism, than as furnishing any evidence of telegony.

In a paper on "Reversion in Birds and Mammals," read before the Royal Society of Edinburgh on December 5th, of which no adequate report has yet reached us, Professor Ewart summed up the results of a long series of experiments in crossing. He showed that reversion does not invariably follow crossing; thus the offspring of Galloway and Highland breeds of cattle might be indistinguishable from pure-bred Galloways, while a cross between a pure white fantail pigeon and a blue pigeon resulted in an intermediate form, with the colour of the fantail and the build of the pouter. On the other hand, he showed that crossing is *often* followed by reversion, as in the case of a cross between an Archangel and a White Fantail, which resulted in a bird exceedingly like a Blue Rock. But we wait impatiently for something better than a newspaper report of these experiments, which are as important as they are interesting.

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## A Study in Hybridism.

It is a far cry from horses to sea-urchins, but the phenomena of hybridism in the two sets of cases seem to be in some respects very similar. In his paper on "The Relations between the Hybrid and Parent Forms of Echinoid Larvae" (*Phil. Trans.* Series B, vol. cxc. (1898), pp. 465-529), Mr. H. M. Vernon shows that various species—*Strongylocentrotus lividus*, *Sphaerechinus granularis*, and *Echinus microtuberculatus*—are not separated by any rigidly-fixed physiological barrier. In fact, there is a general capacity for hybrid-fertilisation. In one case (viz. *Echinus* ♀—*Strongylocentrotus* ♂) cross-fertilisation takes place with greater ease, and produces larvae of larger size than does direct fertilisation.

In most cases the larvae are of the maternal type, but paternal and intermediate types also occurred. But the most interesting result is the evidence of a connection between the relative maturity of the germ-cells and the characteristics which find expression in the hybrid larvae. "The *Strongylocentrotus* ♀—*Sphaerechinus* ♂ hybrid is only formed at the time when the *Strongylocentrotus* ova have reached their minimum of maturity; whilst in the case of the reciprocal hybrid it is shown that as the maturity of the *Strongylocentrotus* sperm increases, it is able to transmute first a portion and then the whole of the hybrid larva from the *Sphaerechinus* to its own type. In other words, the characteristics of the hybrid offspring depend directly on the relative degrees of maturity of the sexual products." It seems as if we might, through such admirable researches as Mr. Vernon's, be able some day to invest with some reality the subtle conception of intra-germinal struggle.

## Variation in a Sea-Anemone.

MR. G. H. PARKER has sent us a note on "The Mesenteries and Siphonoglyphs of *Metridium marginatum*," extracted from the *Bulletin* of the Museum of Comparative Zoology at Harvard (xxx. No. 5).

A second title for this important little paper might have been "A Warning."

The more we learn about the fixed or sedentary forms of animal life the more difficult does it become to separate the species by characters which can be relied upon as being tolerably constant. Many new species, new genera, and indeed some new families must inevitably be unmade again, when we know more about the limits of possible variations which may occur among the zoophytes. The results of Mr. Parker's observations on the single species of sea-anemone (*Metridium marginatum*) bring home to us more emphatically than any previously published papers the hazardous nature of the risk we run in proposing a new specific name for an anemone on the strength of the examination of a single specimen or indeed of half a dozen.

The group Hexactinia to which *Metridium* belongs was until recently supposed to possess two of the ciliated grooves, termed siphonoglyphs; but in *Metridium* only 41 per cent were diglyphic while 59 per cent were monoglyphic. Again, the name Hexactinia implies a certain constancy in the number of the pairs of mesenteries, but in *Metridium* there may be any number from three to eleven pairs, and one indeed was found with as many as fourteen pairs.

It is not necessary to follow Mr. Parker further into his statistics. It is an important contribution to the study of variations he has sent to us, and may have arrived in time to serve as a useful warning to those engaged in the systematic study of the sea-anemones.

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## Sensitive Protoplasm in Plants.

THE streaming movement of the protoplasm in the internodal cells of *Chara* with which botanical students are familiar, is the subject of a recent paper by Dr. Georg Hörmann, issued in book form by Mr. G. Fischer of Jena, under the title "Studien über die Protoplasmaströmung bei den Characeen" (79 pp., with 12 figures; price 2 marks). Dr. Hörmann has studied the arrangement of the currents in the various parts of the plant—leaf, root, and cortical cells—their relation to cell-division and their behaviour under various external stimuli—mechanical, thermal, electrical, and others. From its behaviour to electrical stimuli and comparison with the behaviour of muscle and nerve fibres, the author concludes that the stimulus-conducting substance in a cell of *Nitella*, and in a fibre of nerve or muscle, contains a common fundamental structural item. The nerve fibre substance is purely conductive; that in the *Nitella* cell is associated with a streaming mechanism, that in the muscle fibre with a contractive mechanism.

## A New Species of Revolving Alga.

In his studies on the Plankton of the Illinois river (*Bull. Illinois Lab. Nat. Hist.* v. 1898, pp. 273-293, 2 pls.), Dr. C. A. Kofoid deals very fully with a new species of alga (*Pleodorina illinoisensis*) belonging to the *Volvocineae*. This form consists of an ellipsoidal coenobium, about $\frac{1}{200}$ inch in long diameter, containing normally thirty-two biflagellate cells arranged in five rings around the periphery of the hyaline gelatinous matrix. In common with most if not all the genera of the *Volvocineae*, e.g. *Pandorina*, *Eudorina*, *Volvox*, one pole of the colony is practically always directed forward in locomotion. The cells at this anterior end, however, not only differ from those towards the posterior pole, in that, as in the genera just mentioned, they are provided with larger red pigment spots, but four of them, the so-called vegetative cells, are much smaller than the remaining twenty-eight gonidial cells. The species, therefore, is especially interesting on account of its well-developed structural and physiological polarity. It also confirms the opinion expressed by W. R. Shaw, who founded the genus, that *Pleodorina* occupies an intermediate position between *Eudorina* and *Volvox*, although nearer the former.

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## The Corundum of India.

In these days of the manufacture of artificial rubies it is satisfactory to get reliable information upon the existence and mode of occurrence of the natural stone.

The well-known "Manual of the Geology of India" being out of print, a new edition of the portion dealing with general geology was issued in 1893. The portion descriptive of the economic minerals was not at that time re-edited, but is now to be issued in the form of a series of separate papers, each dealing with a single mineral. The first of these is a most elaborate and exhaustive memoir by Mr. T. H. Holland, Deputy Superintendent of the Geological Survey of India, entitled *Corundum* (Calcutta, 1898, sold at the office of the Geological Survey).

This paper tell us all that is known about the geological occurrence and geographical distribution of Indian Corundum, and gives much information also about the uses to which it is applied. The most important section is that relating to the geological history of the mineral; special attention is called to the fact that it is found associated both with basic and with acid rocks. The author is disposed to the view that in not a few instances it was an original constituent of the rock, just like spinels and other oxides, and not necessarily an alteration-product or a metamorphic mineral. In particular, a felspar rock in the Coimbatore District, Madras, contains unaltered corundum which shows no evidence of secondary origin. It is well known that corundum can be crystallised out from certain slags which contain an excess of alumina.

Mr. Holland's memoir is a remarkably complete and thorough piece of work, and we hope that the Director of the Geological Survey of India will be able to issue a number of monographs worthy of association with this the first of the series.

## Abstracts of Papers before reading.

It is a frequent complaint that the title of a paper to be read before a society, as issued in the circular convening the meeting, affords but little clue to the subject matter. This not infrequently keeps away many who, did they know the subject of the paper, would endeavour to attend and help forward a discussion. Some societies make it a rule to print the paper in advance, and circulate the full paper, or an abstract, to those likely to be interested in the subject, and these societies invariably secure a good discussion, at once useful to the author and to the society. We are glad to note that in the circular issued by the Geologists' Association of London for their December meeting, this course has been followed for the first time. The paper is entitled *Contributions to the Geology of the Thames Valley*, by A. M. Davies, and the abstract of contents of the paper, no doubt supplied by Mr. Davies, is as follows:—

> This is a stratigraphical paper dealing mainly with the beds within the Kimeridge Clay and the Gault in Bucks and Oxon. The most important new observations recorded are (i.) the Presence of Purbeck Beds N. of Haddenham and N. of Towersey; (ii.) the Non-existence of the Gault Outlier mapped N.E. of Haddenham; (iii.) the Presence of Portlandian Beds between the Shotover Ironsands and Kimeridge Clay at Littleworth, near Wheatley. The Author will exhibit maps, sections and specimens, and Mr. J. H. Pledge will exhibit photographs in illustration of the paper.

It is often quite impossible for an author to describe his paper in his title, though such a course is obviously to be desired, and we hope the Association will see its way to continue the abstracts, whenever such a course seems desirable to the Secretary. There are many other societies which might well follow suit.

## A Slight Misunderstanding.

In a recently published volume on "Degeneracy" (Contemporary Science Series, 1898), of which we shall have more to say afterwards, Dr. E. S. Talbot says that "Weismann has practically abandoned the essential basis of his position by admitting that maternal nutrition may play a part in determining variation. He now asserts that the origin of a variation is equally independent of selection and amphimixis, and is due to the constant occurrence of slight inequalities of nutrition in the germ-plasm. As acquired characters affecting the constitution of the parents are certain to affect the nutrition of the germ-plasm, it is therefore obvious, according to Weismann's admission,

that acquired characters or their consequences will be inherited. This is an emphatic though concealed abandonment of the central position of Weismann." But surely this misunderstanding of Weismann's position is inexcusable, and to slump "acquired characters or their consequences" is quite illegitimate. The illustrious author of the "Germ-Plasm" has made it quite clear that there is a very great difference between admitting that the germ-plasm has no charmed life, insulated from somatic influences, and admitting the transmissibility of *a particular acquired character*, even in the faintest degree. The point is this: Does a somatic modification, induced by functional or environmental change, influence the germ-plasm in such a way that the modification, or even a tendency towards it, is transmitted to the offspring? Far from abandoning his position in the "Germ-Plasm," Weismann made it stronger than ever. The first condition of criticism is to understand what one criticises.

## Photography in National Museums.

In connection with our note on this subject last month, it will be of interest to our readers to quote the following remarks from the *Times* of November 24th, in regard to the remarkable collection of facsimile Tudor proclamations now on exhibition at the British Museum:—

> These facsimiles have been beautifully executed at the photographic establishment attached to the University Press, Oxford, which would have been unnecessary if the Museum had possessed a photographic department of its own. Great as is the historical interest of the collection, even this is second to the importance of its exhibition as an object-lesson of the value of photography to great libraries and repertories of archives. There is nothing, from the priceless books and MSS. down to parish registers, which cannot by photography be reproduced and ensured against destruction and decay. With an efficient international system every country might possess every document illustrative of its history or its national life in the past. To this end, however, it is essential that the photographic atelier should be national, and connected with some public institution, for then, and then only, can the photographer be a salaried officer, and expense thus be reduced to a *minimum*. The trifling public outlay would be nearly, if not altogether, covered by public patronage. It is a matter for much congratulation that the first important step to demonstrate the utility and practicability of so great an object should have been made by the British Museum.

These remarks apply equally well to "types" or other subjects in Natural History.

## Physiology of Reproduction.

The experimental examination of the physiology of reproduction in the lower members of the cryptogamic series is a study of comparatively recent date, opening a wide field of important investigation. Most of the definite knowledge we as yet possess of this complex and difficult subject we owe to the labours of Professor Klebs, who,

continuing his well-known researches on the conditions which determine the reproductive processes in thallophytes, has recently published (*Jahrb. f. wiss. Botanik*, Bd. xxxii.) a paper describing the effect of environment on the formation of sporangia and zygotes respectively in *Sporodinia*, one of the Mucorineae. This mould is peculiar, in that both reproductive organs arise from similar primordia, and, according to Professor Klebs, the determining factor, other conditions being equal, is to be found in the amount of transpiration taking place from the surface of the plant, and this in its turn depends naturally on the amount of moisture present in the surrounding air. So rigidly is this the case that in less than twenty-four hours he was able to determine at will whether sporangia or zygotes should be formed, simply by opening or closing the flasks in which the fungus was cultivated on discs of carrot, or on plumjuice-agar, both of which are soils suitable for its growth, though its natural habitat is the decaying cap of the larger toadstools. So long as the relative moisture within the culture chamber does not exceed 65 per cent, sporangia alone are formed, while if it rises above 70 per cent zygotes appear in increasing numbers till absolute saturation is reached. The most favourable conditions for the formation of zygotes are found in the stagnant layer of saturated air which always hangs over the plants in a still atmosphere, and in which any evaporation taking place from the surface of the hypha must be due solely to the temperature of the living substance being slightly higher than that of the surrounding medium. A current of air, even when nearly saturated with moisture, disturbs this layer, and thus tends to promote the formation of sporangia quite apart from any consideration of an improved oxygen supply, which indeed appears to exert but little influence.

The formation of zygotes seems to be unaffected by changes of temperature between a minimum of 6° and a maximum of 26° C., so long as the relative moisture remains constant, but above the higher limit the increasing transpiration induces sporangial development at the expense of zygote formation.

The nutrition of the fungus also influences the formation of reproductive organs. Thus starvation promotes the development of sporangia, not as might be expected of zygotes, and the same result follows the employment of such purely nitrogenous food materials as peptone, albumin, etc. On the other hand, certain carbohydrates such as cane-sugar, maltose, glycerine, dextrine, etc., favour the production of zygotes, while other substances belonging to the same series, like arabinose, lactose, inuline, etc., promote the formation of sporangia alone. The addition of peptone to the above-mentioned carbohydrates does not in most cases influence the result. The acid salts of certain organic acids, especially acid malate of ammonia, tend to the production of zygotes, while the neutral salts of the same acids promote only vegetative growth.

# ORIGINAL COMMUNICATIONS.

## Funafuti: The Study of a Coral Atoll.[1]

By W. J. Sollas, M.A., LL.D., D.Sc., F.R.S., *Professor of Geology and Palaeontology in the University of Oxford.*

By far the largest portion of the untrodden surface of our planet is formed by the floor of the Pacific Ocean. Submerged at an average depth of over 1000 fathoms, it lies out of reach of the geologist's hammer for all time, and for the present at least is inaccessible to the diamond drill.[2] The geology of an almost entire hemisphere is thus the secret of the Pacific.

"It is the nature of a God," Bacon quaintly remarks, "to conceal a thing, it is the glory of a man to find it out," and certainly there would seem to be few secrets in Nature to which a clue has not somewhere been left for those who have virtue to discover it.

The mountainous margins of the ocean, still young and actively moving, may doubtless furnish us with many precious hints, but it is to the multitudinous islands, which in serried rows like the tops of submerged mountain-chains extend across it, that we must turn in search of the true guiding thread.

Some of these islands, like New Zealand and New Caledonia, are in many important respects similar to our own, and seem to be the surviving fragments of a lost continent, which has fallen into ruins and sunk beneath the waves. Others, such as the Sandwich Isles and Fiji, are also of a kind long since familiar to us, clusters of volcanic cones which, like Stromboli and Vulcano of the Mediterranean, rise from the depths of the sea.

In addition to these, however, there exists a third and strange kind of islands, restricted to the torrid zone, and known to the daring mariners of the Elizabethan period as "low" islands, a name well deserved, since few of them attain a greater elevation than many of the

[1] Being the Friday evening discourse delivered before the British Association at Bristol, 1898.

[2] Professors John Joly and Edgeworth David think it may be possible by suitable machinery to bore a hole in the floor of the deep sea.

pebble beaches which fringe our own coasts: few indeed so great, the loftiest summits of most not exceeding the insignificant height of 10 feet. Owing to this fact they are scarcely visible till a ship is close upon them, and the first glimpse of a low island presents itself as a thin dark-green band, which separates the deep azure of the sky from the still deeper blue of the sea: with nearer approach a cream-coloured streak inserts itself below the green and is instantly followed by a line of dazzling snowy white, which is soon recognised as the fringe of surf which marks the boundary of the sea. Sailing nearer, the streak of cream-colour becomes the island beach, and the zone of green resolves itself into a mass of luxuriant vegetation, over which the feathery crowns of the graceful cocoa-nut palms, towering to a height of 80 feet, wave indolently in the sea-breeze.

As the details of this gracious scene, rising like an apparition from the deep, unfold before the eyes, one seems to gaze on some island of enchantment, and with the music of the surf thundering in one's ears one thinks of the Tritons sounding the loud conch, and half expects to "see old Proteus rising from the lea!"

If it be with surprise that we first make the acquaintance of these islands the feeling is in no degree abated with closer familiarity; from beginning to end their whole story is a chapter of surprises.

Mariners soon learned to dread the surf-beaten shores, for they could find no anchorage within a safe distance of the breakers, the sides of the island descending precipitously to great depths within a few hundred yards of the coast; and within this distance a reef of rough and rugged rocks forms the shelving floor of the sea. A barque once driven on to this heels over, with its deck facing the pitiless waves, and is swept clean from stem to stern.

Bristling with dangers on the outside, the island conceals within itself a spacious inner sea or lagoon, into which, through dangerous passages, a ship may make its way, and once there securely ride out the most destructive storm. The island thus differs from most others in being hollow in its midst: a mere rocky rim to a sea-lake, which may be as much as 60 or even 100 miles across, and 60 fathoms deep, though 20 fathoms is more usual. From this feature the islands are known, not only as "low" islands, but as "lagoon" islands. The shores of the lagoon are bordered by a smooth, gently sloping beach of flesh-coloured sand, over which the wavelets fall faintly: and palms and laurel-like shrubs growing down to the water's edge are reflected in its crystal margin.

When the voyager first set foot on this strange new land it was a fresh surprise to him to find it peopled. The inhabitants, usually graceful and prepossessing in appearance and amiable in manners, came timidly forth to welcome him, speaking a language full of soft vowel sounds, which has been aptly styled the Italian of the Pacific. In some cases, particularly when the natives were not red men but

black, they showed less favour to strangers, and the islands sometimes became the theatre of bloody strife.

Besides man, whose presence is an additional problem of the islands, no other mammals are indigenous, their place being taken by various land crabs and spiders of many kinds.

An examination of the rocks of a low island reveals another singular feature: save for a few fragments of pumice, brought from distant volcanoes by sea-currents and cast by the waves upon the strand, they present us with but one kind of material, carbonate of lime, which has been extracted from solution in the sea, and built up into a diversity of solid forms by the agency of organisms,—plants and animals, of which the most conspicuous are corals. Thus but one kind of rock enters into the constitution of the island,—and this is limestone: of granite, slate, sandstone, clay, such as we are familiar with at home, there is none: all is limestone, whatever you see!

The interest of this fact is enhanced by another. Repeated investigation has proved that the island is not merely a residuum, a mortuary of calcareous organisms, but that it is still alive and in active growth. A profusion of gaily tinted corals forms reefs within the lagoon, and the whole of the shelving platform, which descends from the surf to a distance of 20 or 30 fathoms below the sea, is alive with them: this platform is indeed the true growing surface of the island.

Corals, by reason of their considerable size and brilliant colours, first attract the attention of the observer, and hence, although numerous other kinds of creatures collaborate with the corals in the construction of the reef, these islands are known not only as "low" islands and "lagoon" islands, but also as "coral" islands, or more particularly as "coral atolls."

The remarkable discovery that coral atolls consist of the remains of animals and plants of precisely the same kinds as those which are at present adding to its substance, excited general interest, and led to many fantastic speculations, which need not now be recalled. The state of opinion at the beginning of this century may best be learned from the works of the poet-naturalist, Chamisso, who may probably be more widely known as the author of *Peter Schlemihl's wunderbare Geschichte* (The Story of the Man who sold his Shadow) than as an investigator of coral reefs. In a description, which even in the light of the most recent research must still be pronounced excellent, Chamisso (Fig. 1) speaks of atolls as table-mountains which rise steeply from great depths. The summit of the table-mountain is always under water, and is covered by the living reef, which surrounds its margin as a broad platform, and rises to the

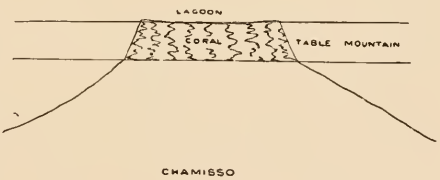


FIG. 1.

level of low tides. Sandbanks resting on this form the dry land. Since, he observes, every particle of the atoll which lies within the reach of observation consists of coral, it is only just to conclude that the whole structure, including the table-mountain, is formed of the same material. Not perhaps a strictly logical conclusion, yet, as events have proved, in the main correct.

Chamisso's opinion was not destined to remain long unchallenged, for two famous French naturalists—Quoy and Gaimard—asserted, as the result of their observations, that the coral-rock of an atoll is only skin-deep, *i.e.* it forms, according to them, a mere superficial crust, not more than about 25 feet in thickness; the rest—Chamisso's "table-mountain"—being, on this view, of volcanic, or at all events of inorganic, origin.

Few of the arguments by which it was attempted to sustain this erroneous conclusion strike one as being very satisfactory, but they include one highly important observation, viz. that reef-building corals do not live at greater depths than 25 feet below the level of low tides. Subsequent inquiry, while fully confirming the existence of a limit, has at the same time extended it down to a depth of as many as 25, or perhaps even 40, fathoms. Yet, even with this modification, the unexpected discovery of Quoy and Gaimard seems to stand in flagrant contradiction to the views of Chamisso. If corals cannot grow below a depth of 25 fathoms, how could they possibly have built up islands of over 100 fathoms in thickness?

The answer to this question, as is well known, was given by Charles Darwin. If we admit the truth of both the apparently conflicting statements, it is obvious that the corals at the base of a reef 100 fathoms in thickness must have been situated within the limit of 25 fathoms at the time they were alive. But in order to bring them within this limit it is only necessary to suppose that the foundation on which they grew originally stood 75 fathoms nearer the sea-level than it does now; or, in other words, that since the lower layers of the reef were alive and flourishing, the ground which supported them has sunk 75 fathoms deeper in the sea. No fact is better established than the rise and fall of islands situated in mid-ocean, and thus there is nothing antecedently improbable in this supposition. But once grant it and Darwin's explanation of atolls naturally follows. Thus let *a* be an island with its summit rising 100 fathoms above the sea; let its shores become peopled with corals, which extend seawards down to the limit of 25 fathoms, beyond which, as we admit, they cannot proceed: a reef is thus started, which will continue to grow, rising upwards till it reaches the level of low tides: when this is attained upward growth will cease, and the reef will begin to pass into decay, from the shore-edge outwards. So long as the island remains stationary, neither rising nor falling with respect to the sea-level, this is practically all that will happen, and the final result is a reef not much exceeding

25 fathoms in thickness (Fig. 2, first stage). But let us next suppose that the island begins slowly to sink into the sea, carrying the reef with it; the upward limit to the growth of the corals will be displaced; they will commence to flourish afresh, and the reef will continue to extend upwards till the level of the low tides is once more encountered, and growth again arrested. This process of submergence and upward growth may of course be repeated indefinitely, and by the time the island has descended 50 fathoms below its original position, the reef will have acquired a corresponding thickness. In such a case the unfavourable conditions to coral growth which prevail on the inner side of the reef, together with the retreating slope of the flanks of the island, will have led to the formation of a channel of sea-water between the reef and the shore (Fig. 2, second stage). Finally, let the submergence of the island continue till it is completely swallowed up by the sea, not a vestige of its summit remaining to mark its place; the upward growth of the corals, constantly proceeding, will bring them once more to the level of low tides, and the result will be the formation of a ring-shaped reef surrounding a central lagoon, or, in other words, of an atoll (Fig. 2, third stage).

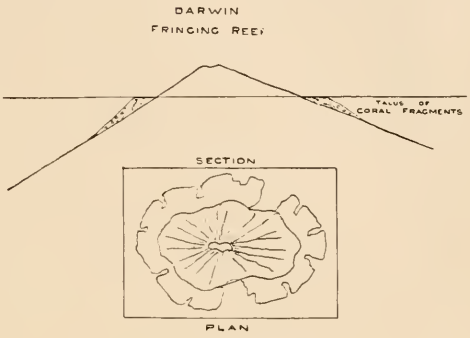

Fig. 2.—First Stage.

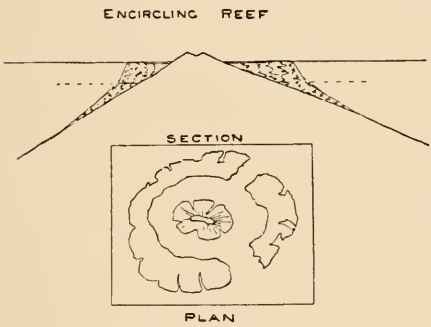

Fig. 2.—Second Stage.

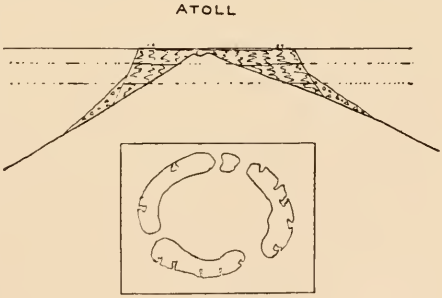

Fig. 2.—Third Stage.

If this hypothetical scheme of the progress of events correspond to the facts, we may expect to find its various stages still represented among the numerous islands of the Pacific; and this, as Darwin endeavoured to show, is clearly the case. The first stage, in which the reef is no more than 25 fathoms thick, and forms a selvage accurately following the margin of the land, is represented by that numerous class known as "fringing" reefs. The second, in which a comparatively thick reef

surrounds an island with an intervening saltwater channel, is illustrated by another class, known as "encircling" or "barrier" reefs. In these, as we might expect, the form of the reef is only remotely related to the contour of the enclosed island, the valleys of which present that fiord-like character so suggestive of sunken land. The last stage is that of the atoll itself.

The excellence of Darwin's theory lies in this, that it explains all the essential features of an atoll on one simple assumption. It is inconsistent with no known fact, and as additional discoveries have been made it has not required to be supplemented by fresh hypotheses. It is not like a Gothic structure, supported by flying buttresses and other *tours de force*, but rather resembles some noble Italian tower, which rises from its base, straight, simple, and self-sufficing. It was no sooner given to the world than it commanded almost universal assent.

Nevertheless it has never been without a rival: even before Darwin published his celebrated work, Ainsworth[1] had suggested a different explanation. He rightly pointed out that Quoy and Gaimard had not established a limit for all reef-building organisms, and that although certain corals, such as they had observed, might be restricted to shallow waters, there might yet be others capable of flourishing at greater depths. If so, these deep-water organisms might be engaged in laying the foundations of an atoll on which the shallower-water forms might erect the superstructure (Fig. 3). This suggestion seems to have fallen still-born, but the notion of "laying the foundation" of an atoll was not destined to perish: it has been revived of late years by Sir John Murray, who, guided by his observations made when on board the "Challenger," was led to suppose that the submerged summits of deeply-sunken islands might be raised to within the limit of 25 fathoms, not by the upward growth of corals but by the incessant downward rain of minute organisms from the surface of the sea. The same agencies which were supposed to be spreading out a layer of chalky mud or ooze over the abyssal floor of the ocean were also imagined as engaged in piling a Pelion of mud on every submarine Ossa (Fig. 4).

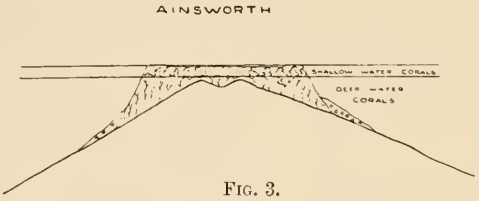


FIG. 3.

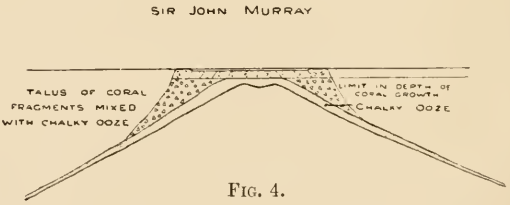


FIG. 4.

[1] G. W. Ainsworth, "Analysis of a Voyage to the Pacific and Behring's Straits, to co-operate with the Polar Expedition, performed in H.M. Ship *Blossom*, under command of Capt. F. W. Beechey, R.N., in the years 1825-28," *Geog. Jour.* vol. i. 1831.

The publication of Sir John Murray's views was followed by a long controversy, in which Darwin's theory was subjected to a most searching criticism. An impartial summary of the arguments arrayed on both sides of the question is given by Professor Bonney, in the last edition of Darwin's "Coral Reefs," and the general subject is treated in the fullest manner by Langenbeck, in a work entitled "Die Theorieen ueber die Entstehung der Koralleninseln und Korallenriffe" (Leipzig, 1890).

So far as the opposition to Darwin's views has come to count among its adherents a number of distinguished thinkers, it can only be regarded as having achieved a certain measure of success: a result not, to my thinking, to be wholly accounted for by the nature of the arguments employed; possibly in this as in similar cases, the ostensible objections are mere weapons of combat, while the real power has lain in the strong and subtle influence exercised by some general current of thought. Such a current is indicated in the tendency to a belief in what is spoken of as the Permanence of Continental Areas and Oceanic Basins.

According to Darwin, every atoll marks the site of a vanished island, but the atolls of the Pacific are so numerous that if one imagines all the islands they represent as summoned back from the "vasty deep" and restored to their original position above the sea, they will constitute a very considerable tract of land, and this situated in the very middle of the Pacific Ocean. Such a prospect could not fail to be unpleasing to those who believed in the immutability of the ocean.

Of late years, however, this doctrine of "permanence" has begun to look a little threadbare. In a theoretical restoration of the distribution of land and sea during the Jurassic times, Neumayr has treated it with scant consideration, since he represents the North and South Atlantic, as well as the Indian Ocean, as then to a great extent occupied by land, and it is now very generally supposed that this land did not disappear to make way for existing seas till a comparatively late period in the history of the earth. Bold as Neumayr showed himself in the treatment of these oceans, he had not the temerity to take liberties with the Pacific. This he and geologists in general are disposed to regard as having maintained its existing features from a very early period: of this ocean, and of it alone would they exclaim, "Such as Creation's dawn beheld, thou rollest now."

Darwin's theory, as we have seen, does not hesitate to recall to existence land in the middle of even this ocean; this is its unforgivable offence—it lays sacrilegious hands on the Pacific, and thus attacks the doctrine of "permanence" in its stronghold.

While the recent controversy on Darwin's theory was at its fiercest, and both sides seemed equally persuaded that the truth was theirs and must prevail, it occurred to me that a simple solution

might be obtained by sinking a bore-hole through some well-characterised atoll, and thus obtaining specimens of the material of which it is composed, down to a depth considerably greater than that at which corals are supposed to build. How would this illustrate the question? Allow me to employ a homely illustration: buyers of cheese are not, I presume, naturally more suspicious than other persons engaged in trade, but they are unwilling to trust too much to mere outward appearance; they are not inclined to adopt the argument which commended itself to Chamisso in a parallel case, that because there is good cheese on the surface it must be good cheese all through: consequently by means of a boring instrument, called a scoop, they make a hole through the cheese and bring out a core or cylindrical rod, in which the several strata of the material, if there be more than one, are displayed in their true thickness and natural position. The atoll is our cheese, which we propose to sample with a complicated kind of scoop called a diamond drill. This should provide us with a core in which the various layers of the coral reef should be faithfully represented. Should Darwin's theory prove correct, the core will contain the remains of reef-building corals as far down as the reef extends; if, on the other hand, Sir John Murray's explanation make a nearer approach to the truth, layers of chalky ooze will be present at depths greater than that of the limit of coral growth (Fig. 5).

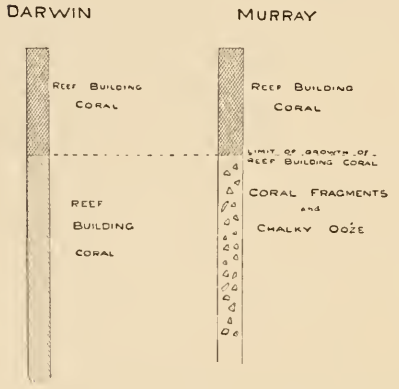


FIG. 5.

No one who has any notion of the extraordinary thoroughness with which Darwin attacked this as every other problem that he investigated, will be at all surprised to learn that the same solution had already occurred to him, and in a letter to A. Agassiz (May 5, 1881) he sighs for "some doubly rich millionaire, who would take it into his head to have borings made in some of the Pacific and Indian atolls, and bring home cores for slicing from a depth of 500 or 600 feet." As the wished-for millionaire did not appear to be forthcoming, it appeared to me that the boring might be achieved in another way, by a method very familiar to this Association, I allude of course to a "Committee." On approaching Professor Bonney with a suggestion to this effect he warmly entertained the proposal, and in 1891 a strong Committee, including the most distinguished supporters and opponents of Darwin's theory, was formed, having for its object the investigation of an atoll by boring and other means.

Through the kind offices of Professor Stuart of Sydney we obtained from the Government of New South Wales the offer of the free loan of

a diamond drill. Our next step was to select an island for investigation. This was rendered an easy task through the invaluable assistance afforded by Admiral Wharton, whose extensive knowledge of coral-reefs renders him the most formidable of Darwin's opponents. At his suggestion our choice fell on Funafuti, one of the Ellice or Lagoon Islands, situated in the middle of the Pacific (lat. $8\frac{1}{2}^\circ$ S.), seven days' sail northwards of Fiji. No better selection could possibly have been made. Not only is Funafuti an atoll of unexceptional character itself, but it belongs to a family of atolls all of equally unexceptional character; and these again to a system which includes the Gilbert and Marshall Islands, all of them excellent atolls. So far as these are all distinguished by the same characters, whatever may be found true of Funafuti will apply to all the rest.

The labours of the Committee of the British Association were then taken over by a Committee of the Royal Society, at whose request the Admiralty generously assigned to our assistance the "Penguin," one of H.M. gunboats, commanded by Captain Field, and stationed in the Pacific for exploring purposes. The Royal Society furnished funds to defray expenses, and the direction of the expedition was placed in my hands: two volunteers, Mr. Gardiner of Cambridge and Mr. Hedley of Sydney, were with my permission to accompany me.

We joined the "Penguin" and left Sydney on 1st May 1896, taking with us a boring party which had been selected for the work by Mr. Slee, the Government Inspector of Mines and Drills. Its foreman, Ayles, had acquired great reputation in the colony by his success in conducting boring operations of exceptional difficulty. On 21st May, after three weeks' voyage, we heard the welcome cry "Land ho!" and Funafuti was seen on the horizon. The ship was steered for the southern entrance; this was safely made, and we steamed into the noble lagoon. Flying-fish spurted from under our bows, and zig-zagged in their darting flight around us; here and there in the midst of the blue waters green and purple shallows marked the site of growing coral patches. On the starboard side lay the beautiful island of Funafuti proper, its pale sands ablaze in the light of the tropical sun, its groves of palms cool with a refreshing green. A boat put off from the beach manned by a crew of copper-coloured natives, their black hair crowned with wreaths of *Gardenia* and *Hibiscus* flowers. They were soon swarming over our sides, bringing with them the solitary white trader of the island, who safely piloted us to anchor within a mile of the shore. Captain Field and a party immediately landed, and we went at once to pay our respects to the king, who, notwithstanding the narrow limits of his realm and the smallness of his nation, which numbers only some 240 souls, we found to be every inch a king. His Majesty received us with gracious dignity, led us into his palace, one of the few stone huts on the island, and seated us by his side on the daïs, which consisted of packing-cases.

The chief men sat round the walls on the floor, and smiling damsels, with large black eyes, ivory white teeth, and long black tresses floating loose, shyly presented us with freshly opened cocoa-nuts to drink, a civility which as inevitably attends a call in Funafuti as the afternoon cup of tea at home. We told our errand, and received permission to choose a site for boring operations. We then requested that a house should be built for us, and were promised that this should be done for the modest sum of £6. The reception ended, we proceeded to choose a site for the boring and for landing gear, and marked out the plan of our house; it was to measure 15 by 20 feet. We were anxious to have the building of this put in hand at once, and were assured that it should be ready for us by the afternoon of the next day. The East is supposed to be more fertile in promise than performance, and our expectation was that we should see this house when we did see it. Judge, therefore, of our surprise when on passing the same spot the day after we found a substantial structure already standing there. It had grown up like Aladdin's palace in a single night! The whole population had been employed on the work; the men had cut down trees and shaped them into poles, sunk these in the ground, and bound them together into a solid framework; the children had been set to gather palm leaves from the forest, and the women had woven these into mats, which were used to form both the walls and thatch of our dwelling. The result was an excellent house which served all our needs, protecting us from sun and storm during our residence of nearly three months. Not a nail was driven in its construction, all the joints being firmly made with cocoa-nut cord.

After contemplating the work with great satisfaction I left for a stroll, and returning an hour after was aghast to find our new house surrounded with smoke and flames! To my great relief it turned out that the conflagration proceeded from the surrounding bush, which the thoughtful natives had purposely set alight to prevent its taking fire by accident.

The work of landing gear and erecting machinery was set about vigorously; the crew of the *Penguin* toiled all day heroically in the burning sun, refreshing themselves at sunset in swimming matches with the natives; progress was so rapid that by 3rd June, not quite a fortnight after landing, the boring party were already at work (Fig. 6). So far all our plans had been carried out with expedition and success, and since "things done well and with a care" are said to "exempt themselves from fear," we may now safely leave our miners industriously boring while we take a walk across the island. Standing on the shore of the lagoon near the site of our boring, it is just possible to catch a glimpse of the palms on the opposite side, some ten miles away. The beach slopes so gently, that although the tide falls only about five feet it leaves a wide expanse of sand uncovered; this is a perfect warren of shore crabs (*Calappa*), which scurry along like blown thistle-down

and vanish into holes with mysterious suddenness. It is at night that these are most active, when they dig deep burrows in the sand, casting up conical hillocks at the entrance nearly a foot high, which give the beach the appearance of a miniature encampment. The sand is the famous "coral" sand; but on picking up a handful for nearer inspection we are surprised to find that it contains scarcely any coral; and so far from consisting of detrital material, it is almost entirely composed of the shells of Foraminifera, two species predominating, *Tinoporus baculatus* and *Orbitolites complanata*. From specimens collected on other atolls by the late Professor Moseley, and preserved in the University Museum at Oxford, it would appear that the sand at Funafuti

Fig. 6.—The Site of the First Boring, with Derrick and Machinery.

is by no means singular in this respect, and the term "coral" sand is only another instance of the "lucus a non."

The lagoon beach ends in a tiny cliff about a foot in height,[1] to the very edge of which sparse turf and vegetation of a larger growth extends; the land to which this cliff is boundary consists chiefly of small fragments of coral and shells of Foraminifera; it rises a little so as to attain a maximum height of 3 or 4 feet above high-water mark. In breadth it varies considerably, and where broadest the native village stands, with the church, large enough to contain the whole population, all church-goers, the school, mission-house, and palace. A row of graves, made tomb-like with slabs of coral, runs down the middle of the main street. The whole of this sandy flat is covered

[1] This applies to that part of the islet on which our house was built: in some places more considerable cliffs are met with, *e.g.* on one of the northern islets of Funafuti called Amatupu, where a conglomerate of coral pebbles forms steep faces some six feet or more in height.

with rich forest growth (Fig. 7), cocoa-nut palms in all stages, from the young plant just sprouting from the shell to the ancient of the groves, 80 feet in height, bearing heavy clusters of ripe fruit beneath its crown of feathery fronds; pandanus, with its strange adventitious roots and truculent sword-shaped leaves, broken in the middle; the laurel-like Nono (*Morinda citrifolia*); and the "Nya" tree (*Pemphis*), with its heavy stem of hard red wood, and delicate foliage. Ferns abound, and some brightly coloured flowering plants; an *Abutilon*, which puts forth fresh blossoms day by day; and a handsome bean, which trails through the forest, bearing large heart-shaped leaves and heavy racemes of lilac flowers.

The great robber-crab (*Birgus*), which feeds on cocoa-nuts and

FIG. 7.—Forest scene in Funafuti.

pandanus fruit, is at home here, and may be seen climbing the cocoa-palms by night. Other land crabs scramble through the fallen palm leaves which thickly strew the ground. Many of these are of the hermit kind, and one of them has a curious habit of croaking like a frog when captured. But no part of the island is free from land crabs; like rats and mice they are the universal scavengers; they undermined our house, attacked our tinned provisions, and one could not sit down to eat a cocoa-nut without some of these weird creatures gathering round to pick up the fallen crumbs.

As we continue our passage across the sand, the scene rapidly, even abruptly, changes its aspect; the place of the forest so rich and varied is taken by a grotesque growth of "Nya" trees, whose stubborn contorted trunks, strangely at variance with their dainty foliage, bar the way; struggling through these, one enters upon a savage plain, "horrid" with rugged fragments of blackened coral, and cumbered here

and there with huge boulders of coral rock some tons in weight.[1] At low water this desolate region is dry and burns in the sun, but as the tide rises sea-water oozes up through holes in the ground and covers it with shallow pools. Few animals live in this desert; spiders, that infest the "Nya" trees, and mosquitoes, that lie greedily in wait by day as well as night, are the chief that I bear in mind. Proceeding lengthwise along this plain, which lies in the middle of the island, it broadens out and passes into a muddy swamp, planted by the natives with taro, a delicious substitute for potatoes, and bananas, which one still reflects upon with pleasure; their fruit was our

FIG. 8.—*Heliopora* in the foreground, *Porites* beyond the Mangrove Swamp, Funafuti.

chief luxury, and we willingly paid for it at the somewhat exorbitant price of four fathoms of calico a bunch.

Farther on beyond the plantation, the depression becomes still wider, forming an extensive flat, partly margined by mangrove trees and *Hibiscus;* this was known to us as the mangrove swamp (Fig. 8). It is an interesting corner of the island. The floor represents the upper surface of a deal coral reef, composed partly of great masses of *Porites;* their flattened summits, standing some 8 or 10 inches above the floor, give them the appearance of a row of stepping-stones, and mark what was the level of low tide at the time the reef was living.[2] Radiating from these blocks as from a nucleus are vertical plates of the "blue

[1] One of these measured 6 feet by 5 feet by 4 feet.

[2] The last episode in the history of the island appears to have been a slight elevation of some four or five feet: at least I was led to this conclusion from evidence furnished by the "dead reef" of the mangrove swamp, by the "sea-stacks" or pinnacles of coral-rag of the tidal platform, and by the steep cliffs which in some of the islets border the lagoon.

coral" (*Heliopora coerulea*) which extend outwards, branching as they go, for a distance of 3 yards or more. Overlapping the reef lies a layer of consolidated coral breccia; it has suffered much from erosion by the sea, and bounds the inner side of the depression in cliffs 3 or 4 feet in height. A sheet of clear green water covers the swamp at high tide, converting it into a shallow lake, which as the tide falls empties itself through deep holes in the floor into subterranean passages, which freely communicate with the outer sea. The northern end of this depression is closed by coral breccia, and overgrown with mangroves; but farther on it recommences and extends through the remainder of the island, almost as far as its northernmost extremity, forming a discontinuous

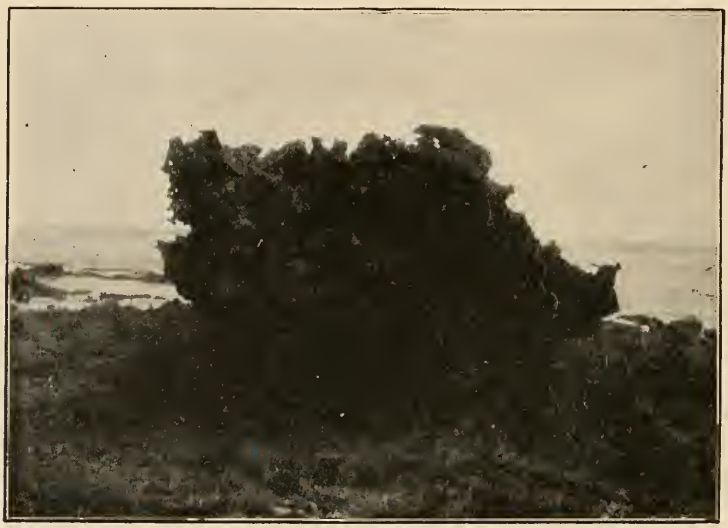

FIG. 9.—A Raised Pinnacle of Coral.

narrow trough bordered by steep cliffs. This trough, and the depression to which it belongs, owes its origin in some degree to solution by sea-water.

We have deviated from our walk across the island to follow the course of its central depression, let us now return and resume our traverse. The blackened fragments of coral, resembling nothing so much as the clinkers of lava which cumber the slopes of Etna, continue seawards, and are loosely piled to form a gently rising ascent; so loosely piled that they often topple over at a touch, and afford very uncertain or even dangerous foothold.

Walking circumspectly, therefore, up the slope we soon reach the summit of a long ridge, and find ourselves looking towards the Andes, some thousands of miles away over the broad waters of the Pacific Ocean. We stand on the top of the "storm-beach," the loftiest region of our island, at the imposing altitude of 10 or even 15 feet, according

to the state of the tide. On the seaward face the storm-beach descends somewhat rapidly, and near its foot a sheet of hard consolidated coral-rag emerges from under it, to form a gently sloping platform, over which the tide ebbs and flows. In places this tidal platform rises in low cliffs, ridges, and pinnacles [1] of fantastic shape (Fig. 9), but for the most part it presents itself as a sheet of limestone, smoothed and polished by the wearing action of the waves. For about fifty yards from its seaward edge it is hollowed into a broad shallow depression (Fig. 10), not deep enough to be called a channel, and finally swells into a narrow rounded rim formed by the growth of a pink-coloured calcareous seaweed known as *Melobesia*. Beyond this rim, which projects above the sea at low

Fig. 10.—Funafuti on the Ocean Side.

tide, lies the growing surface of the reef, which is constantly submerged, so that under no circumstances are the corals which thickly cover it at any time exposed to the air.

Deep chasms gash the edge of the tidal platform, the continuation inland of the lanes of clear sea which wander through the growing reef; in these chasms a few corals may generally be found, their polypes sometimes brilliantly coloured and in full expansion.

The calcareous alga, previously alluded to as *Melobesia*, forms the lips of these chasms, and by its luxuriant growth may more or less completely roof them over, generally leaving one or more apertures, which act as blow-holes.

The ocean side of the reef is one of the pleasantest parts of the island: a cool breeze almost always blows there; and, under the welcome shelter of the palms and pandanus which crowd the summit of the storm-beach, one may watch the beautiful and impressive spectacle

[1] See note 1, p. 29.

below (Fig. 11): the ocean, of a deep majolica blue, rolls inwards in majestic waves, which suddenly grow gigantic as they approach the shore, towering in a wall of water above the reef; and then spring with a furious roar into a confusion of white foam, which seethes about the madder-tinted margin of *Melobesia*, rushes through the chasms of the tidal platform, and often spouts up through the blow-holes with sudden and explosive violence, like a kind of marine geysers. It is only on calm days that the extreme margin of the reef can be approached with safety. Such is the violence of the breakers that the tidal platform presents the appearance of an almost lifeless desert; a few green and brown seaweeds, little fish darting in the pools, occasional sea-snails with dense shells, and a few hermit-crabs heavily armoured, are all that

FIG. 11.—Tidal platform and margin of reef.

is seen at first glance. All the inhabitants of the tidal platform seem to stand in dread of the sea; even the active shore crabs (*Grapsus*) are afraid of it, and only venture in when inspired by their greater terror of the human form; even then they cling tenaciously with their many legs close to the sides of the rocky shore, and sidle off to land directly they fancy the enemy's back is turned.

The observer who trusted to first impressions, and judged the platform by its outer aspect, would fall into grievous error; it is by no means so dead as it seems. On breaking off a fragment with a hammer a new world of life is revealed; the rock is tunnelled through and through, as closely as it can be mined, by a variety of animals, which have taken to an underground life as a protection against the sea: worms, shell-fish, crabs, sea-squirts, and barnacles are to be found in these subterranean dwellings; they constitute a specialised

fauna of marine troglodytes, which might, if we wished to add to the burden of nomenclature, be designated the "Cryptone."

After this brief description of the superficial features of the atoll, we may next endeavour to trace the history of that part of it which rises above the sea and properly constitutes the land. The sheet of hard coral-rock, which we mentioned as cropping out beneath the storm-beach, can be traced into the interior of the island, where it forms the floor of the central depression; and again to the lagoon side, where it emerges to form the floor of the lagoon, and in many places the beach, or as well even a low line of cliffs. In the little islet of Pava, north of Funafuti, it is seen to extend continuously from one side of the land to the other—from the ocean to the lagoon.

We may therefore fairly conclude that this sheet of rock forms the solid base on which the land above it rests. It is composed mainly of slabs of coral, lying not quite horizontally, but overlapping like the tiles of a roof, with a slight inclination towards the ocean side of the reef.

These fragments have evidently been derived from the outer zone of growing coral. Before the land, as it now exists, was formed, the waves were incessantly engaged in tearing off fragments from the coral zone, and driving them across the reef into the lagoon, till a thick sheet of debris was the result. This became consolidated as it formed, partly by the growth of incrusting calcareous algae, and now forms the solid floor of the island.

Masses of broken corals, torn up and driven inland by the breakers, continued to accumulate after the formation of the floor; and thus that great pile of coral clinkers, which forms the storm-beach, has been and is still being built up.

On the other side, the wavelets of the lagoon have washed up smaller fragments of coral and foraminiferal shells, and thus the strip of land which borders the lagoon, and on which the village of Funafuti stands, has been produced.

The middle of the island—the great central depression including the taro ground and the mangrove swamp—is the remains of the original solid platform left exposed between the storm-beach on the one hand and the lagoon land on the other. Thus all that portion of Funafuti which stands above high tide has been cast up from the ocean and the lagoon, and this beautiful island, like another Aphrodite, has been born with the foam from the waves of the sea.

If this be the true history of the island, how then did it acquire its inhabitants? (Fig. 12). Did they climb upwards, like the corals, as the island was submerged, or did they arrive as flotsom and jetsom of the sea. As regards the natives there can be but one answer—they came by boat. In former days the Polynesians possessed excellent sea-going craft, in which they were accustomed to make long voyages, steering by the stars and other signs in the sky. They well knew how to preserve food by drying, and thus had no difficulty in provisioning for a cruise. The routes they followed in passing from island to island are

gradually becoming known to us; and have been indicated on a chart by Professor Haddon. Considering the remarkable similarity of language which characterises all Polynesia, from New Zealand on the south to the Sandwich Isles on the north, there can be little doubt that the migrations of these peoples must have taken place comparatively recently, and judging from tradition one might conjecture within the last seven or eight hundred years.

Thus long before the illustrious townsman of this city, John Cabot, had anticipated Columbus in his famous voyage to America, these navigators, whom we libel with the name of savages, were venturing on equally arduous explorations, with still more imperfect means at their

FIG. 12.—A Group of Natives, Funafuti.

command. It was not often, however, that long voyages of over a thousand miles were made of set purpose; too frequently they were the result of accident, as when frail canoes were overtaken by a sudden storm and driven at the mercy of the winds, sometimes to perish miserably, sometimes by good hap to land on undiscovered shores.

The Funafuti people seem some of them to have entered the island with intent, others are mere waifs and strays cast away by shipwreck on the reef. The prevailing stock is Samoan, with an admixture of Tongan. In bygone times the Tongans used to make periodical descents upon the island, after the fashion of the Vikings in early English history. The Tongans, however, came not only to kill but to eat their foes, a proceeding not wholly unintelligible among a people who knew absolutely of no other kind of meat. In justice to the copper-coloured races of Polynesia I hasten to add that cannibalism

was seldom the custom of this folk; wherever it is met with it may be taken to indicate the influence of black blood. So far as we know cannibals are almost always black people.

Returning to the boring party, which we left busily engaged. For nearly three weeks they worked by shifts continuously night and day, but at the end of that time, when the bore-hole was only 105 feet deep, their most arduous efforts failed to advance it further. The difficulties opposed by the nature of the ground—a mixture of flowing sand and obdurate boulders—were such that neither the good-will of the workmen nor the ingenuity of Ayles, the foreman, could contend against them, and there was no alternative but to abandon the undertaking.

Thinking that there might be a better prospect of success on the ocean side of the reef, we determined to make a fresh attempt there, and in two days, without the help of wheels and in a country without roads, we succeeded in transporting the bulk of our twenty-five tons of machinery across the island to a fresh site. The new boring commenced in hard rock and at first deepened rapidly. Before long, however, it entered a mixture of sand and boulders similar to that we had previously encountered, and after attaining a depth of 72 feet further progress became impossible. We left the island on 30th July, and on reaching Fiji had the mortification to learn that we had passed on the way a ship coming to our assistance with a fresh supply of machinery, which our friends in Sydney had promptly despatched on hearing of our difficulties.

Our attempt to penetrate the reef had proved a failure, but it was not wholly without result. It had revealed the nature of the material with which any subsequent attempts at boring would have to contend, and it had added one more surprise to the history of atolls, for no one had suspected that for a depth of over 100 feet the island would be found to consist of more sand than coral, or in other words, that the organisms which play the chief part in the construction of a coral reef are not corals, but Foraminifera!

The expedition had other objects in view besides boring. The next in importance was the investigation of the atoll by sounding. This was accomplished with complete success by Captain Field. Other atolls had been sounded before, but never before had an atoll been sounded with such accuracy and completeness as was Funafuti on this occasion. The form of the floor of the lagoon was made more exactly known than that of most lakes in the British Isles. The slopes of the flanks of the atoll were determined in four different directions, approximately at right angles to each other and running about N., S., E., and W. A study of these enables us to frame a clear picture of the general form of the atoll. It is a conical mountain with an oval base situated at a depth of about 2000 fathoms, measuring 30 miles in length by 28 in breadth. It rises at first with a very gentle slope but gradually grows steeper as it ascends (Fig. 13), till at a depth of 400 or 500 fathoms it begins to present precipitous faces, and above 130 to 140 fathoms is

crowned by the almost vertical cliffs of Chamisso's "table-mountain," which, as he rightly divined, is of a similar nature from base to summit. All this is coral reef; how much more may be so it is impossible in the present state of our knowledge to say.

The general feeling of disappointment with which our failure to bore through the reef was received was fully shared by our friends in Sydney. Determined not to be put off with a first rebuff, they promptly commenced to make arrangements for a second attempt, and last year (1897) an expedition again left Sydney for Funafuti, this time under the direction of Professor Edgeworth David of the University of Sydney. Under his leadership the boring proved a complete success. The reef was penetrated to a depth of 697 feet, or 116 fathoms. Thus Darwin's wish has now been more than satisfied. The core

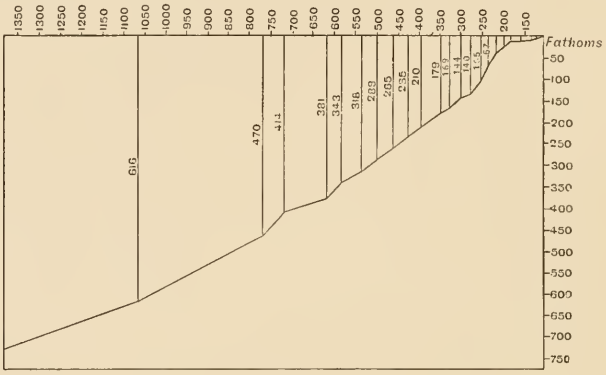


FIG. 13.

brought up was sent over to this country, and is now in the hands of Professor Judd for investigation. Till he has completed his report it would be premature to enter into details, but from a general examination, made without the aid of the microscope, I think I may fairly venture to say this much, that the material brought up from the boring, and of which the reef is composed, presents much the same general character throughout, and so far supports Darwin's theory; that layers of chalky ooze, such as on Sir John Murray's hypothesis we might have expected to find in the lower parts, are conspicuously absent; and finally that it presents no trace of volcanic material.

On whatever side judgment may ultimately be given in this question, the thanks of the scientific world must undoubtedly be conceded to Sir John Murray for having disturbed a decided opinion from its slumber, for having awakened a fresh interest in Darwin's theory, and in thus leading to renewed investigation, which is both adding to our knowledge and suggesting fresh inquiry.

The sand showing little trace of consolidation which was noticed in our boring down to 100 feet is maintained in Professor David's boring down to about 100 fathoms, and it is not a little remarkable that

material so loosely aggregated should be able to sustain itself in slopes of as much as 80°, such as characterise the flanks of Funafuti. It is important, however, to observe that none of the borings yet made have been sunk through the true growing substance of the atoll. They have commenced on the lagoon side of the true coral reef, and the deeper they have descended the more remote they have become from the ocean flanks. The possibility exists, and should not be overlooked, that a great part of the material passed through in the bore-holes represents deposits of the lagoon and of the fragmentary debris driven towards it by the breakers.

It will be observed that Professor David's bore-hole does not traverse the whole thickness of the table-mountain; judging from the soundings, it would have to descend 20 or 30 fathoms deeper to do this, and it would seem likely that the material obtained from this last 20 or 30 fathoms might surpass in interest all the rest. Our friends in Sydney fully appreciate this, and are so bent on probing this question to the utmost, that they have already despatched, at great pecuniary risk, an expedition to make a third attempt on Funafuti, and this time to carry the bore-hole right through the table-mountain.

The boring party is at this moment at work on the island, and before many weeks have elapsed we may expect to receive tidings of their success. A great stride will then have been taken towards a final determination of the long-standing controversy on the origin of atolls.[1]

We eagerly await the result, which will inform us whether these central oceanic islands are ancient remains of land which have plunged beneath the sea and are renewing their youth, or whether they are among the latest products of our planet, aspiring mountains which have scarcely yet succeeded in their struggle upward to the light of day; whether they are, as has been said, "a garland laid by the hand of Nature on the tomb of a sunken island," or whether they may not be a wreath of victory crowning a youthful summit on its first conquest of the main.

[1] The critical point has been passed. (See *Nat. Sci.* xiii. p. 362.) According to the news from Funafuti (Sept. 6) the boring attained a depth of 987 feet, or 147 feet below the base of the steepest cliff. The material passed through was coral limestone. It is of interest to observe that, soon after passing the bottom of Professor David's bore-hole, loose unconsolidated deposits ceased to be encountered, and the drill passed with comparative facility through a hard limestone containing numerous well-preserved corals. A crux of all theories of atolls is the lagoon. On Darwin's theory its explanation follows naturally from the fundamental assumption. Sir John Murray has to supplement his hypothesis by a separate explanation, and proposes to account for the lagoon by solution. In this connection the success which has attended an attempt of the present expedition to bore into the bed of the lagoon is most welcome. The boring was made from the deck of H.M.S. "Porpoise," commanded by Capt. Sturdee, and after passing through 101 feet of water sank 144 feet into the deposits of the floor. The first 80 feet were found to consist of the calcareous alga *Halimeda* mixed with shells; the remaining 64 feet of the same material, mingled with coral gravel. This alga is universally distributed over the floor of the lagoon, as proved by an examination of the material obtained by Capt. Field in sounding, and since it contains a certain percentage of magnesium carbonate we are led to expect that the formation of dolomite will be found to stand in some connection with the transformation of lagoon deposits.

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# Professor Weldon's Evidence of the Operation of Natural Selection.

By J. T. Cunningham, M.A.

In his Presidential Address before Section D of the British Association, at its last meeting, Professor Weldon gave an account of his investigations of the variation of the shore crab in Plymouth Sound. The Address is printed in the number of *Nature* for September 22, 1898. The evidence it contains is considered by the author to prove, firstly, that a certain change has taken place in one character of the animals in question in the period of five years, and secondly, that the change has been caused by a selective destruction of individuals, due to the increasing turbidity of the water in which they live. These conclusions, if correct, would be of very great importance, and for this reason it is necessary to examine and consider carefully the evidence on which they are based. I have examined the evidence, and it does not seem to me to furnish the proof required. I propose to state here the reasons why I am unable to accept Professor Weldon's conclusions.

He gives, in the first place, three determinations of the mean frontal breadth of the crabs, expressed in terms of the carapace length taken as 1000. In other words, he measures the frontal breadths of individual crabs, and expresses them as so many thousandths of the length of the carapace. The three determinations are one for each of the years 1893, 1895, and 1898. It is important to observe that the mean frontal breadth in crabs collected at the same time varies very rapidly with the length of the carapace. The mean was therefore determined for every ·2 mm. of carapace length in crabs from 10 to 15 mm. long. The larger the crabs, the less is the mean relative frontal breadth. For example, in crabs 10·1 mm. long in 1893, it was 816 thousandths of the length; in crabs 14·9 mm. long, it was 751 thousandths—a decrease of 65, omitting decimal fractions.

For groups of the same carapace length the mean frontal breadth was less in 1895 than in 1893, and less in 1898 than in 1895, but the numbers measured in 1898 were admittedly very small. Here arises the first objection to the argument. The dimension was less in crabs of the same size, but every dimension observed in one year was

present in the other years, in crabs of a different size. The character investigated is in a state of transformation during growth. Crabs of a given size, say 12·1 mm., were narrower in 1895 than those of the same size in 1893, but crabs of 1895, a little smaller, had the same frontal breadth as those of 12·1 mm. in 1893. In other words, a given mean frontal breadth was never wanting in any of the three years, but was found associated with a different size of the crab. From this fact alone it is impossible to base any argument concerning selection on the variations observed and recorded in Professor Weldon's Address. The variations investigated are variations of the mean frontal breadth in proportion to carapace length, and it is not even suggested that any particular variations were absent, or less frequent, less common, in one year than another. It is merely stated, and may be accepted as a fact, that each degree of the dimension was found in smaller crabs in the later years. It does not matter whether we say that crabs of a given size were found to be narrower in frontal breadth, or that crabs of the same frontal breadth were found to be of smaller size. In consequence of the diminution of the frontal breadth with increase of size both statements are equally true.

Before proceeding to the explanation given of the cause of the change, it may be noticed that in a footnote a previous hypothesis put forward by Prof. Weldon concerning the selective destruction of variations in this character is now abandoned, and we are informed that it is to be replaced by the results contained in the Presidential Address. That previous hypothesis was that the narrowest as well as the broadest fronted crabs were killed off, so that the range of variation in the older and larger crabs was reduced. Of this previous conclusion a very eminent Darwinian said at the time, in a letter to *Nature*, that its demonstration "deserved to rank among the most remarkable achievements in connection with the theory of evolution." It is evident now that this admiration was premature; the process was hypothetical, had never been demonstrated at all, and the hypothesis is now abandoned in favour of a kind of selection which is quite inconsistent with that formerly suggested.

The measurements, to which I have referred above, all relate to male crabs; the change in females during the same time is said to be less in amount though in the same direction.

Prof. Weldon is confident that the change is due to a selective destruction caused by increase in the amount of sediment in Plymouth Sound. According to his argument the turbidity of the water in the Sound is increasing because the breakwater obstructs the scour of the tide and removal of the silt, while china clay is brought down by the rivers from Dartmoor, and the increasing population, dockyard, and shipping imply the passage of more sewage and refuse into the Sound.

In order to ascertain whether the broader fronted crabs were killed by sediment, a number of specimens were put into a large vessel of water in which fine china clay was placed and kept suspended by an

agitator. After various periods of time the dead crabs were separated from the living, and the mean frontal breadth of those that died was distinctly greater than that of the survivors.

A similar result was obtained when crabs were treated in the same way with fine mud obtained from the shore on which they were found. Professor Weldon concludes that the action of mud on the beach is the same as in the experiment, and that here we have a case of natural selection acting with great rapidity because of the rapidity with which the conditions of life are changing.

Lastly, there is another experiment in which some hundreds of crabs were kept, each in a separate bottle until it moulted, when of course it became larger. At first some crabs died and these were found to be those with broadest frontal regions. This was therefore a selective destruction without the influence of mud, and was proved by a separate experiment to be due to the putrefaction of the particles of food with which the crabs were fed. So that organic putrefaction and bacteria have the same effect on the crabs as inorganic sediment. The surviving crabs moulted, and after they had hardened their new shells they were killed and measured, and their dimensions compared with the dimensions they had before the moult, as ascertained from the cast shells. Before the moult the mean frontal breadth was below the normal for the size, afterwards it was above the normal for the new size, and some specimens were very remarkably broad. The reason for this is stated to be that the same crabs were measured at the two sizes, while in nature during the interval the broadest specimens had been killed off.

With regard to the experiments with china clay and mud, I would point out that the smallest and youngest crabs are the broadest fronted, and that the result might easily be explained on the hypothesis that the smallest and youngest crabs succumbed first. Professor Weldon believes that death occurs because a narrow frontal breadth renders the filtration of the water entering the branchial chamber more efficient. But filtration would depend on the absolute size of the apparatus, and this is getting larger as the crab grows larger; therefore the larger crabs ought to be killed first, and in nature all the adults ought to be killed. On the other hand, if relative frontal breadth has the importance supposed, the selective destruction ought to be greatest on the very young crabs, and if it kills the broadest fronted of those between 10 and 15 mm. long, it ought to kill all those under 10 mm. in length, since these are all broader. In this case there would be no crabs over 10 mm. in length.

In fact, Professor Weldon argues that selective destruction has in five years produced a diminution of not more than 18 units in the frontal breadth of crabs of the same size, while his observations show that there are living on the same shore at the same time multitudes of crabs of different sizes whose frontal breadths differ by

as much as 65 units. If we took into account crabs less than 10 mm. long and adults, the difference in their relative frontal breadths would be much more than 65 units. As a proof that the cause of death in the experiments with fine clay or mud was the entrance of the latter into the gill chamber, Professor Weldon states that the gills of the crabs which died were generally covered with the sediment, while those of the survivors were not. But the dead crabs were taken out at the end of the experiment, so that they had been in the muddy water some time after death, while the survivors were taken out alive. It is possible, therefore, that the sediment found its way in after death, and the evidence is no proof that filtration was more efficient in one case than in the other.

From Professor Weldon's own point of view the result of the moulting experiment does not bear the interpretation he puts upon it. He states that the crabs measured before the moult were on the average narrower than those on the shore. Therefore, immediately after the moult these same crabs would still have been narrower than their fellows on the shore which were of the same size before the moult, and moulted at the same time. Consider 500 crabs, say all 10 mm. in length, in the bottles, and 500 of the same size on the shore. The former have narrower frontal regions. Both undergo a moult at the same time; if there is no difference in the growth of the two groups the crabs in the bottles must be still narrower, for they are the same crabs in another stage. But those on the shore are exposed to selective destruction after the moult. This cannot well reduce the mean frontal breadth below that of those in the bottles, for it now has a narrower breadth to act upon, and in the previous stage it was not able to reduce the frontal breadth so low as that of those in the bottles. Of course I understand that after the moult some of those on the shore are supposed to be killed off, and none of those in the bottles. But the 500 on the shore were broader to start with by actual observation, and if they could survive with this greater breadth at the size before the moult, I fail to see why many should be killed off when they reach a larger size and a narrower relative frontal breadth. Moreover, there are, in fact, multitudes of crabs on the shore a little smaller than those in the bottles after the moult, and of the *same* mean frontal breadth.

It seems to me, as I suggested in a letter to *Nature*, that all Professor Weldon's observations may be completely explained by variations in the amount or rate of growth. The difference in different years would be at once explained if the amount of change in frontal breadth was constant for each moult, while the amount of growth was variable. The fact is, that in 1893 crabs of a given frontal breadth were larger than in 1895 and 1898 ; and I have shown that the summer of 1893 was exceptionally fine and warm. Either the warmth alone, or warmth and food together, very probably made the crabs grow more in that year for the same number of moults. On this view the broad-fronted crabs

died in the experiments with clay and mud because they were younger and weaker. In the same way the crabs that moulted in the bottles possibly grew more than those in the sea, because they were kept in warmer water and supplied with more food. Therefore they were, after the moult, larger than those in the sea of the same relative frontal breadth.

My suggestion is also in accordance with the fact stated by Professor Weldon that there was less change in female crabs than in male. If the difference of frontal breadth was the cause of selective destruction, it is difficult to see why it should act on one sex more than on the other. On the other hand, it is known that male crabs are larger when adult than females, therefore they grow faster, and their growth would be more affected by changes in the conditions of life in different years.

Professor Weldon states, in reply to my letter in *Nature*, that he has tested my suggestion and found that it is not in accordance with the facts. Apparently he has examined the measurements of the crabs which moulted in his experiment, and found that those which increased most in size at the moult did not differ in frontal breadth from the mean of those of the same size on the shore, more than those which grew least. According to Professor Weldon's original statement, the crabs that moulted were on the average broader than those of the same size on the shore. According to my suggestion, the broadest individuals should be those that grew most at the moult. This my opponent denies. I have asked him to produce the figures, but he declines to do so until all the figures relating to the matter are ready for publication.

As a proof that in the experiments with mud or clay the youngest crabs did not die first, he has instanced an experiment in which, out of many crabs 10 to 15 mm. long, only four survived, all under 13 mm. But he does not say that these four were the narrowest, as they ought to have been on his own hypothesis. If they were, they may also have been the oldest, or they may have been, if my second suggestion (see below) is correct, those which had grown fastest and were therefore the most vigorous. If they were not narrower than all the others which died, the experiment does not support Professor Weldon's conclusions.

Mr. Walter Garstang has challenged my suggestion in another way. In a letter to *Nature* he contends that exuviation is essentially connected with the process of growth in Crustacea, and that "in assuming similarity of size in young shore crabs to indicate an equal number of moults, Professor Weldon is quite in accord with our present knowledge of the subject." It would take up too much space to enter here upon a discussion of the question whether ecdysis is essentially related to increase in size or not, but I may refer to certain facts as to the relation between size and the number of moults.

In 1884 the late Mr. George Brook published a paper on the "Rate of Development of the Common Shore Crab" (*Ann. and Mag. Nat. Hist.* vol. xiv. pp. 202-207). He recorded a number of observations and measurements of young crabs kept alive for various periods in

captivity. Some of the specimens grew at nearly the same rate, that is to say, the increase in size was nearly the same after the same number of moults. But in other specimens this was not the case.

In August 1883 he obtained five specimens in the Megalopa stage. He gives the dates of the moults of two of these, and the sizes after each moult. These specimens he distinguishes as A and B. The latter, B, was later in moulting than A; its fifth moult (after it had attained to the crab-form) occurred on April 23, 1884, while A moulted for the fifth time on March 8. The crab, after the fifth moult, was in the case of B 5·7 mm. long, in the case of A 4·44 mm. long, a difference of 1·3 mm. In February both of these crabs had moulted four times, and there was a difference of ·34 mm. between them. This proves that crabs of different sizes on a given date in some cases have moulted the same number of times. Supposing the change in relative frontal breadth to be constant, or to be less variable than the increase in size, then these two crabs of different sizes would have nearly the same relative frontal breadth. To take another case from the same paper. A crab N was 12·5 mm. long in July 1882. After five moults, in the following May, ten months, it was 37 mm. long. Another crab Y was 12 mm. long in September 1882; after five moults, in the following June it was 45 mm. long, the moults having only occupied nine months. Here we have a difference of no less than 8 mm. in the carapace lengths of two crabs after the same number of moults, although their original difference was ·5 mm. In one case the increase was 24·5 mm., in the other 33 mm., a difference of 8·5 mm. in the increment. This shows how great may be the difference in the amount of growth after a given number of ecdyses. Supposing the larger crab did not cast its shell again for a month, we have the above difference of growth after not only the same number of moults, but after the same interval of time. If we look at the measurements recorded, we find that after four moults the crab Y was nearly of the same size as the crab N after five moults. Supposing then that the change in frontal breadth at each moult is nearly constant, is not proportional to the change in size, then the crab Y, though of the same size as N, was one moult behind it, and therefore of distinctly greater frontal breadth, since the relative frontal breadth decreases at each moult. Thus, if the crabs in 1893 in Plymouth Sound grew as much in a given moult as Y, while in 1895 they grew at the rate of N, then the crabs of the same size in the two years would represent different numbers of moults. Unfortunately Mr. Brook did not measure the frontal breadths, and therefore I cannot carry the argument further, but it seems to me extremely probable that the change in the frontal breadth of crabs which reached a given size in four moults, would not be the same as in those which reached it in five moults. If that is correct, all Professor Weldon's evidence proves is that the crabs grew more at each moult in 1893 than in 1895, and the evidence he has produced

has no bearing on natural selection at all. Of course I admit that some of the crabs in Mr. Brook's experiments grew nearly the same amount in the same number of moults, but the point is that others did not.

Another curious point brought out by Brook's records is that, when a given number of moults results in greater growth, the time occupied by those moults may be more or less than in the case when the growth was less. Thus, in the second of the above cases the larger crab took less time for the same number of moults than the smaller crab, in the former case the larger crab took longer time than the smaller for the same number of moults. It is evident that the relation of the growth of a crab to age and ecdysis is very complicated.

Professor Weldon has however informed me privately that he finds the abnormality of frontal breadth to be very nearly independent of the growth-rate during a moult, but that the difference, such as it is, shows that the crabs which grow most are on the whole very slightly narrower after their moult than those which grow least, in comparison with those of the same size in the sea. In consequence of this it has occurred to me that perhaps my first suggestion was wrong, and that the effect of conditions on growth is exactly opposite to that described above. It is certain that both the frontal breadth and the carapace length are growing, and that the carapace length grows faster, since the larger crab has an absolutely greater but relatively narrower frontal breadth. Now, if any circumstance such as temperature or food increases the growth, it may affect the carapace length more than it affects the frontal breadth. In that case the crab that grew most in one moult would be the narrowest. For example, suppose that a crab has a carapace length of 1000 units, and a frontal breadth of 800 units. Suppose that under ordinary circumstances in two moults the carapace length increases to 1050 and the frontal breadth to 820, then the crab is of course larger and relatively narrower. Now, suppose that the growth is so hastened that it reaches the same carapace length in one moult. The effect on the growth of the frontal breadth may not be so great, and thus this dimension may only increase in the one moult to 815; thus the crab of the same carapace length is narrower because it has grown twice as fast. Perhaps this view of the law of growth is more probable than the other.

But it will be said this new suggestion could not fit the same facts as the other. It would fit the same facts of observation, though of course the supposed conditions of life must be different. The conditions on this hypothesis were more favourable to growth in 1895 and 1898 than in 1893. In the experiment when the narrowest fronted crabs of those of the same size survived, it would have been because they were younger but more vigorous than those which had not grown so fast, the rapidity of growth being a sign not of weakness but of strength. In the moulting experiment the amount of growth would on this hypothesis be less than in crabs on the shore, and therefore the

crabs would be broader than those of the same size on the shore. Whatever the influences of conditions on growth may be, I feel convinced that the facts observed by Professor Weldon are due to such influence, and for the reasons I have given, not to selective destruction. I have shown that either of my two hypotheses would explain the facts better than the selective destruction which Professor Weldon believes to be the cause.

In 1895 Professor Weldon stated that his statistical results could not be considered to be established until the law of growth of the crabs had been experimentally investigated. Yet he now publishes with equal confidence quite different results from statistical data, although in the meantime he has produced no evidence whatever concerning the relation between variations of growth and change in the relative frontal breadth.

In reply to my letter in *Nature* Professor Weldon urged that, if my suggestions were correct, crabs gathered in January would be narrower in the frontal region than those of the same size gathered in August, and he had found that those gathered last January were not narrower than those gathered last August. But such a result does not follow from my view. According to Mr. Brook's experiments crabs above 10 mm. in length would be at least a year old, and therefore had been under the influence of all seasons of the year, and shore crabs hatch their eggs from February or March till late in summer. Therefore crabs taken in January have not necessarily grown under more unfavourable conditions. On the other hand, the observation of Professor Weldon that the crabs taken in January were not narrower than those taken in August shows that there had not been a continuous diminution in frontal breadth from August 1893 to August 1898, as he stated in his Address.

Whether either of my suggestions in explanation of the observed facts be correct or not, I think I have shown that the evidence on which Professor Weldon bases his conclusions is quite inadequate to establish them. The change described is not, if terms are used correctly, a change in the character of the species, but merely a change in the rate of development. The variations investigated are not individual differences, since each individual in the course of its growth passes through each one of the variations in its own person. It has not been shown that the change has gone on continuously for five years, or that it has taken place only in waters where there is much mud. If tadpoles of the same size were found to have shorter tails in one year than in another, few biologists would draw the conclusion that the result was due to the selective destruction of those with the longest tails. The more probable explanation would be that those with the shorter tails were in a more advanced stage of their metamorphosis.

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# Biological Analogy and Speech-Development.

By Henry Cecil Wyld.

Professor Paul, in the chapter headed "Sprachspaltung" of his luminous book "Die Principien der Sprachgeschichte" (2te Aufl: Halle 1886), places on record his astonishment that students of the science of language do not oftener seek analogies for the phenomena of speech-development from the processes of development which obtain in organic nature. It is indeed to be lamented that philologists do not often make themselves familiar with biological method, and with the main results attained for evolutionary thought in this domain of science. For, indeed, did students of language possess an organised system of thought upon lines of evolution, as applied to speech, there can be small doubt that not only would linguistic science be placed upon a broader and more philosophic basis than it rests on at present, but from the nature of the field of inquiry evolutionary thought as a whole would receive a most suggestive contribution. It is indeed possible that many general questions, upon which at the present moment the minds of biologists are clouded with a doubt, would be set at rest one way or the other, and this or that theory of the modes of evolution would be either exploded or firmly established. For, whereas biologists have to trust to an imperfect geological record for a large part of their evidence, the philologist may observe in the course of five hundred years a change in language, much more significant and considerable than the same number of millenniums could effect upon species of animals or plants.

Professor Paul then ventures upon an analogy, and a fascinating one it is, between the factors and conditions which determine the evolution of species and those which govern change in language.

Unfortunately not only does this analogy share with others the fate of containing a fallacy, but the whole passage is, in my opinion, fraught with no small confusion of thought and phrase. His case is briefly this—

Development in animal life depends upon two main factors: first, the characters of the parents; second, the environment to which the organism is subjected after its birth. The results of these factors are: from the first (the hereditary group) similarity with the parents, and

therefore adherence to the general characters of the type; from the second, the possibility, within certain limits, of departure or divergence from the ancestral type.

With regard to speech, we find likewise two main factors of development: on the one hand, the speech of each individual is acquired from and moulded by the commerce of his associates, whom we are to consider as his linguistic parents as it were, while on the other hand, it is modified by his independent personal peculiarities and idiosyncrasies.

For speech, as for species, the former of these two factors, or groups of factors, is by far the more important, and that because every modification of the nature of the individual (speech or organism) which diverges from the originally *inherited* tendency, determines the direction of variation for the succeeding generation.

But we must not forget, says Professor Paul, that there are important differences between the conditions under which an animal on the one hand, and a language on the other, is born. For whereas the direct influence of the parents ceases for the animal from the moment of its birth; the speech-parents of an individual exert their influence from his first infant babblings down to the latest moment of his life, although in a varying degree.

Again, whereas the animal or plant has but two, sometimes only one parent, the speaking individual learns from hundreds of different speakers, and has therefore an almost infinite number of "strains" infused into his speech. This I believe to be an accurate statement of Professor Paul's suggestive comparison.

But if we examine it closely, and endeavour to use it as a starting-point from which to think out the process of speech-development, the analogy fails us at every turn, and appears, in fact, to lead us into one blind alley after another. In the first place, it is surely a confusion of terms to speak of an inherited character as a "factor" of development. It is rather a fact of life. The factors which may modify the inherited characters are left untouched and unnamed. If we were to say that an organism inherits tendencies to change in a certain direction, we might then, I conceive, talk of this inherited tendency as a "factor" in evolution or development; but Professor Paul seems to take for granted that all inherited characters make for a conservation of type, so that they are thus, strictly speaking, not factors of development at all. In passing, we may note that it is scarcely biologically conceivable that characters acquired in the lifetime of the individual, mere modifications of structure from the action of the environment, should form a starting-point for permanent variations in the species, or, as Professor Paul puts it, that "every modification of the nature of the individual which diverges from the originally inherited tendency determines the direction of variation for the succeeding generation." But we need not pause here to discuss the moot question of whether acquired characters are inherited, but may pass on to criticise the justness of the rest of

the above comparison between language and species. It appears that we are to consider the speakers from whom we learn our language as corresponding to the parents on the biological plane, and the speaker's own individual temperament of mind and body as corresponding to environment. Now, as a matter of fact, the great majority of speakers with whom we come in contact must, as regards their influence, be divided into at least two classes: the persons who, in the first place, teach us the beginnings of speech, and the persons who afterwards modify in one way or another the speech which we already possess. The first class are all those speakers who help to build up our mode of speech down to the moment when, so to speak, our speech crystallises and becomes characteristic of ourselves. This moment is, of course, an abstraction, but there comes a time in the life of each individual speaker when his habit of speech is to all intents and purposes formed, and after which he practically ceases to "learn." Until this happens, we may, if we choose, consider our associates as our speech-parents. This is, as it were, the period of gestation, and when it is over our independent life begins; independent, that is to say, just as far as any life can be independent of its environment. The associates of this second period of life, in which we are independent of parents, must be considered rather as part of the linguistic environment. Professor Paul's contention that the animal has only two parents is, of course, without weight, since the individual organism is the product, not of a single pair, but of millions of ancestors, any one of whom, remote or near, may assert his supremacy in the building up of his descendant. In this way the physical structure of an animal is the result of factors certainly no less numerous, complex, and varied than those which go to the making of a man's manner of speech.

It seems to me improper to drag the mental and physical constitution of the speaker into this analogy, because to do so is to go off into the domain of biology and psychology, and we are comparing linguistic facts with facts of organic life, and not these with themselves. But, it may be asked, are we to leave these facts altogether out of consideration as regards language and its development? The answer is, certainly not, and the whole difficulty arises from having made a false analogy at the outset. There is an unfortunate habit amongst students of language, of considering this as if it had an existence apart from the speakers. Professor Paul in his brilliant book has done more, perhaps, than any other philologist to break through this mode of thought, but he has apparently lapsed into it again in the chapter on "Sprachspaltung."

The safest way to think of language is as a habit of body expressing a habit of mind. Sounds, which are the external side of speech, are due to certain modes of movement of the vocal organs. Vocabulary, or system of nomenclature, is the association of sounds with emotions and ideas. Grammar, a further mental process, is the expression of

the perceived relations of emotions and ideas with other emotions and ideas. The very articulation of sounds, as Professor Paul admirably and clearly points out, is due to certain psychological processes. Thus each division of language depends upon mental states, and the sounds themselves which body these forth are the direct results of organic movements. It becomes clear then that speech, like other habits, and perhaps in a more subtle and complex manner than any other habit, is learnt in the beginning by imitation, and is modified and moulded by each individual in the mint of his mental and bodily personality. According to this view, therefore, the factor which makes for conservation of speech type is the necessity for intelligibility: that which makes for variation is divergency of temperament, psychological and physiological, among the speakers. But although the dominating function of these two factors is respectively change, and a hindering of change, they each contain the potentialities of the converse. For, since there is undoubtedly an objective tendency on the part of a community as a whole to slowly change their speech in the same direction, it is clear that a wilful conservatism on the part of the individual would defeat his object of being intelligible to his fellows. So, again, the tendency of the individual to vary in habits of body and mind will have a limit set to it by conditions of life, of climate, of race, of education, of ideals, and of morals, which he will undergo in common with the other members of the society to which he and they belong.

It is these cross currents of tendency which complicate the whole problem of the evolution of speech, and make it difficult to state the question with clearness and completeness. Still, these difficulties must some day be met, and it cannot be denied that so far nobody has made an adequate attempt to meet them. It were devoutly to be wished that some scholar of sufficient courage and learning would rend the veil, and light up the darkness beyond. The archives of the Science of Language are at this moment like a magnificent collection of stuffed animals and carefully-preserved beetles, most of them indeed roughly classified and ticketed. We contemplate them with no little enthusiasm, we have numbered the hairs on the heads of many of the larger mammals, have gathered the dust from the wings of the butterflies; but if you ask us whence they came, or question us as to their life, their growth, and death, we cannot tell. Like the learned ancients who beheld the flies in amber, "we wonder how the devil they got there"; in fact, our philosophy of origins is that of Topsy. In the meantime philologists must watch and pray for the coming of their Darwin and their Weismann.

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# Animal Symmetry.

By A. T. Masterman, B.A., D.Sc., F.R.S.E.

The least attention to the subject of the symmetry of organisms will serve to convince one that the present state of our knowledge, or, to speak more accurately, our present method of expressing the facts, is far from satisfactory. In most modern text-books professing to deal with animal morphology the subject is either left severely alone or is dismissed with a few remarks upon so-called "radial" and "bilateral" symmetry, illustrated by one or two well-known examples. Thus Hatschek [1] divides the Metazoa into Protaxonia and Heteraxonia, relying mainly upon the position of the blastopore; and if we go further back we find that the heterogeneous group Radiata owed its name to the emphasis of a particular prevalent form of symmetry.[2] Standard works upon botany devote more attention to this branch of morphology, as must needs be the case when the structure of flowering-plants is described.

As to a correct definition of the term "symmetry," when applied to organisms, there appears to be a considerable divergence of opinion. Thus, many botanists regard the symmetry of a flower as meaning the similarity of its constituent parts about one or more axes or planes, the term polysymmetrical being, from this point of view, regarded as equivalent to "actinomorphic," or, in other words, symmetry about a median axis, whereas "zygomorphic" implies a symmetry about a plane. Sachs draws distinctions between monosymmetry and polysymmetry on the one hand, and between bilateral and multilateral symmetry on the other, pointing out that the former are special cases of the latter. His term "monosymmetrical" alone corresponds to bilateral symmetry as usually accepted amongst zoologists. Geddes, in his suggestive article on "Morphology," [3] remarks in this connection that "botanists since Schleiden" have contented "themselves with throwing organisms into three groups—first, absolute or regular; second, regular and radiate; third, symmetrical bilaterally or zygomorphic. . . . Burmeister, and more fully Bronn, introduced the fundamental improvement of

[1] "Lehrbuch der Zoologie." [2] Cuvier, "Règne animal."
[3] Geddes, P., "Encyclopaedia Britannica," 1885, p. 843.

defining the mathematical forms they sought not by the surfaces but by axes and their poles."

By certain French and English botanists the terms "symmetry," "symmetrical," and "asymmetrical" have been employed in a very different connection, namely, to express the numerical correspondence or otherwise between the constituent elements of concentric whorls.

A definition which would include all these ideas would have to express merely a vague order of some kind, involving repetition, as existing between the parts of an organ or organism.

The term is used commonly amongst zoologists in a sense resembling the wider usage amongst botanists. Thus the acknowledged distinction of radially and bilaterally symmetrical organisms rests upon this interpretation of the term symmetry. But the so-called "zonal symmetry" instituted a fresh conception of the term "symmetry," which, if accepted, would again require a vague definition. In the former case we deal with the definite arrangement of the parts of an organism about a median geometrical centre; in the latter is introduced the idea of a serial repetition of certain parts along an axis, thus leading to a confusion between the terms "segmentation" and "symmetry." Haeckel[1] has recognised this distinction by applying the term "antimere" to the unit of so-called "radial" symmetry, and "metamere" to the segment or "zoonite."

Geddes[2] clearly shows how the study of symmetry implies the attempt to force mathematical or mechanical conceptions upon the organic world, and indicates that the attempts of Moseley and Goodsir in this direction were based upon many analogies which, "savouring more of the *Naturphilosophie* than of sober mathematics, could only serve to discourage further inquiry and interest."

The analogy drawn from time to time between the organism and a crystal and the parallel between Haeckel's "promorphology" and crystallography have an underlying basis of truth, but they are liable to be misconstrued if carried too far. The material basis of vital phenomena, namely protoplasm, is essentially amorphous (or truly asymmetric, see later) as regards any properties inherent in itself; and the form assumed by any mass of it, constituting for the time being an individual, must depend entirely upon the particular environment at that time. In highly differentiated individuals the exact extent of the direct effect of environmental changes is a disputed question, but by whatever particular path the process may be effected it may be taken as a truism that an organism tends to change its form in relation to changes of environment, and that while in the lower types the change is effected many times in the history of the individual, in the higher types it is with equal certainty effected in the history of the species. These considerations do not apply to the crystal.

On the other hand, the crystal and the organism are each isolated

[1] Haeckel, "Generelle Morphologie," Berlin, 1866. [2] *Loc. cit.*

masses of matter, and hence have similar stereometric relationships. Here the analogy ends.

Haeckel's classification of animal symmetry has not received the amount of recognition it deserves from zoologists, partly because it is encumbered with a copious and somewhat unwieldy nomenclature. Jaeger's text-book of zoology may be noted as one of the very few which give anything like an adequate discussion of the subject.

The other classic work dealing with the subject is Spencer's "Principles of Biology" (vol. ii. chaps. vi. and xiv.), which appeared at about the same time as Haeckel's work. It is difficult to understand why his classification of symmetry has not been more widely adopted, though it is regrettable that a classification equally applicable to both plants and animals, if anything rather more to the latter, should be more or less hidden in a chapter on the *Morphological Differentiation of Plants*. His classification, differing little from that proposed below, is followed by a characteristic attempt to show the inter-relation between the symmetry of an organism and its environment. Though written over thirty years ago, there is scarcely a sentence that does not apply with equal force at the present time. His three types of spherical, radial, and bilateral symmetry are equivalent to the centro-symmetry, axo-symmetry, and plano-symmetry, the first three types instituted below. The justification for alteration in the nomenclature will be there stated.

Amongst smaller works we may mention the essays of Dr. Amans,[1] dealing with the form of animals exhibiting aquatic locomotion. He follows up the "ovoid" form from the sphere through circular, elliptical, unisymmetrical, and asymmetrical ovoids, the bilaterally symmetrical ovoid being correlated with the highest form of locomotion. He thus recognises the great importance, previously pointed out by Spencer, of the form of locomotion in determining the form of symmetry.

Haeckel, twenty years after his former work (in 1887), returned to the question of animal symmetry in his "Radiolaria."[2] He points out that text-books such as those of Claus and Sachs give an insufficient treatment of the subject, and whilst recognising the difficulty of the nomenclature, he adheres to the principles formerly stated. He here gives four principal types of ground-forms which correspond to those of Spencer; and the first three are very nearly equivalent to the three types proposed below, with centres composed of a point, a straight line, and a plane respectively. His main subdivisions, the Homaxonia and Polyaxonia in the first group, and Monaxonia and Stauraxonia in the second group, do not appear to me to differ sufficiently in kind nor in degree to warrant the subdivisions,

[1] *Comptes Rendus*, cv. 1887, and *Ann. Sci. Nat.* vi.

[2] Challenger Report, vol. xviii.

as the sphere and the circle are usually treated geometrically as a polyhedron and a polygon respectively, but with an infinite number of bounding sides. Other differences will be noticed by a detailed comparison.

In comparing the various meanings attached to the term "symmetry" the only really consistent definition must be somewhat as follows:— Symmetry of an organism is the *system of arrangement of its constituent parts in relation to each other and to a certain geometrical centre, which may be called the centre or nucleus of symmetry.*

Animal symmetry is therefore merely a particular character of form, and as such it must be in the same direct relation to environment as any other morphological character. In order to illustrate this relation to its greatest extent one must adopt a classification of symmetry which will bear direct comparison with the different grades of environment. Only in so far as it enables us to correlate form and environment can a classification be regarded as "natural," just as a classification of species can be regarded as "natural" only when it expresses the true history of the changes of correlated form and environment, or the course of evolution, in the genus or family.

Our conceptions of space being based upon the three dimensions, we naturally turn to the geometrical conceptions and their classification in order to find a "natural" classification. In the inter-relation of organism and environment and its determining influence, we find that the three dimensional axes correspond to the lines of development, and hence they are never inclined to each other. In other words, we find no true "clinometric" forms, and we have only to deal with the "orthometric." The stereometrical representatives of the types will, therefore, all belong to the "right" series, and to this extent there is a marked difference from the types of crystallography.

Scattered throughout modern zoological literature we may find statements that "radial" symmetry is correlated with or induced by a sedentary or pelagic habitat, and others to the contrary, the latter mainly relying upon the fact that there are, at the present state of evolution, some sedentary or drifting organisms which show little if any "radial" symmetry. It should not be necessary to furnish proof that the symmetry of an organism tends to conform to the symmetry of environment. Assuming this statement, it is necessary to seek for a classification of environmental as a preliminary to that of organismal symmetry.

The widest division of symmetry of environment must depend upon the three dimensions, and this consideration leads us to the following divisions of animal form:—

1. *Centro-symmetry.*—This consists essentially of repetition of parts in three dimensions, hence the centre of symmetry is a point, the point of intersection of the three dimensional axes (Fig. 1). The minimum number of secondary centres of symmetry will be four, and the

predominant number will be six, corresponding to twice the number of dimensions. The geometrical representatives of these two conditions are the tetrahedron and the octahedron respectively, and the other possible numbers are represented by the cube, dodecahedron, and the icosahedron. An absence of secondary centres results in the sphere, hence the term "spherical symmetry," as used by Spencer. All truly centro-symmetric organisms should conform to the types of the regular polyhedron or the sphere, but are not necessarily confined to the latter. This type agrees also with the centro-stigma or sphaero-typic group of Haeckel.

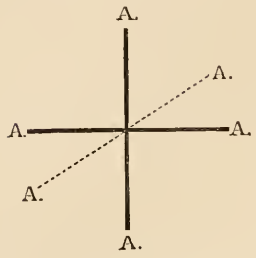

FIG. 1.—Centro-symmetry. (6 A.)

The organisms exhibiting this form of symmetry consist of a great number of Protozoa, such as the Heliozoa, and a large proportion of the Radiolaria and Reticularia. In addition to these, we may include certain of the Flagellata, as *Uroglena*, *Synerypta* and *Pandorina*, and Acinetaria. A great number of the Protozoa also assume this form of symmetry as a transitory phase in their life-history (encystment). Apart from adult organisms, the great majority of the eggs of higher types conform more or less closely to this type, as also do the so-called blastula larva of the Metazoa, and the gemmules of Porifera, *e.g. Spongilla*.

We thus notice that the animal organisms which exhibit centro-symmetry are confined to the adult phases of the Protozoa and to the early ontogenetic phases of many higher Metazoa. It is evident that, with the form and constitution of the earth as it is at present, it is impossible to conceive of an environment in itself which is centro-symmetric, that is, symmetrical round a point. The fact that organisms dwell upon the surface of a sphere (oblate spheroid, to be more accurate), causes a constant heterogeneity in one dimension which, through the physical factors of gravity, and in most cases light, will be a constant factor in the induction of a certain amount of heterogeneity in organisms. Hence, in the attempt to correlate structure and environment, the existence of centro-symmetric organisms would appear to be a difficulty. This, however, is only apparent, for the effect of environment upon the organism is dependent not only upon the environment, but upon the manner in which the organism, so to speak, allows itself to be subjected to it. Thus Spencer points out that centro-symmetric organisms are usually free to rotate about their centre, either actively or passively, thus eliminating the effect of axo-symmetry in the environment.

Again, if the organism be for a time removed from the influence of its environment by the formation of a coat, a cyst, or a shell, the influence being reduced to zero in all directions, the centro-symmetric form will be assumed.

Thus we may connect the occurrence and origin of centro-symmetry in organisms with two special conditions of existence:—Firstly, the free rotation, active or passive, of an organism about a central point; secondly, the free encysted or encased condition in which the living matter is more or less removed from the influence of its environment. The spherical form implies homogeneity, whereas the polyhedral types imply heterogeneity, as limited by certain definite numbers of secondary centres of symmetry.

Haeckel, taking the geometrical types of the sphere and the regular polyhedra as enumerated above, shows how each type is represented by certain of the Radiolaria. A similar classification could probably be effected in each group of the Protozoa, though perhaps not quite so completely.

The experiments of Quincke, Bütschli, and others with emulsion "foams," and of Roux with oil-drops, tend to emphasise the great importance of the physical environment in determining the morphology of the asymmetric and centro-symmetric Protozoa, and the early ontogenetic stages of Metazoa respectively.

2. *Axo-symmetry.*—This consists essentially of repetition of parts in two dimensions, and hence the centre of symmetry is formed by the third dimension, which is the axis of symmetry.

The minimum number of secondary centres of symmetry is reduced to three, and the dominant number will be four, corresponding to twice the number of the like dimensions. Above this the number is practically unlimited, as represented geometrically, from an equilateral triangle and a square through any regular polygon to the circle.

This type of symmetry is called "radial" by Spencer, but the term has been applied generally to include both this type and the preceding, as according to the meaning it might. On the other hand, if "spherical" symmetry is retained for the preceding, the equivalent term for "axo-symmetry" would be "circular symmetry." Haeckel classed this type with the next as Protaxonia, whilst Hatschek limits this term to the present type. Haeckel's later term is Centraxonia. Haeckel's types do not exactly correspond to axo-symmetry, for he includes those forms in which the two transverse axes differ from each other, whilst these are here included in the Plano-symmetric.

There are two conceivable types of axo-symmetry, namely, true or mon-axo-symmetry, and di-axo-symmetry (see Figs. 2 and 3).

In true axo-symmetry there is heterogeneity between the two poles of the dimension of symmetry; in di-axo-symmetry they are homogeneous.

True axo-symmetry is geometrically represented by a right regular pyramid and cone, di-polar by a right regular prism and cylinder.

The di-axo-symmetry is a rare phenomenon, mainly because axo-symmetric animals have their dimensional axis perpendicular, and hence this axis is subjected to the perpetual heterogeneity of environ-

ment already referred to. The conditions for the acquirement of di-axo-symmetry are a horizontal position of the main axis of the organism with free rotation about this axis. Certain Radiolaria, such as the Acantharia, according to Haeckel, present this ground-form, and the Amphistomina, certain of the Ciliata, and to some extent the Thaliacea, may serve as further examples.

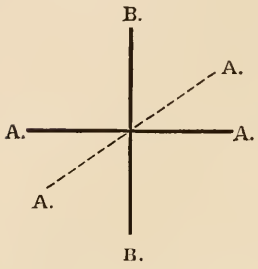

FIG. 2.—Di-axo-symmetry. (4 A+2 B.)

True axo-symmetry is exhibited by a great number of animals, besides nearly all the plant kingdom. It is found in many of the Protozoa. It is the fundamental type of all the Porifera, Coelentera, and Echinoderma. Traces of it are found in the Polyzoa, Cirripedia, sedentary Annelida, and Tunicata. The Cuvierian group of Radiata depended mainly for its institution upon the presence of this type of symmetry. We have only to add that, in the ontogeny of many of the above, and in that of higher Metazoa, this type is represented by the gastrula.

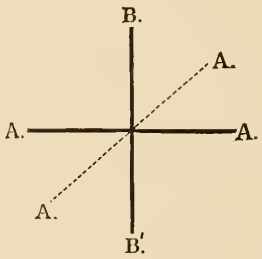

FIG. 3.—Axo-symmetry. (4 A+B+B'.)

A study of the above phyla will show that this axo-symmetry occurs in precisely the environmental conditions in which, from a theoretical point of view, we should expect to find it. They fall into three groups :—

1. Sedentary.
2. Pelagic floating (plankton in restricted sense).
3. Free pelagic with axial rotation.

(1) *Sedentary.*—It is usually granted that axo-symmetry is correlated with a sedentary existence. This statement is in no way invalidated by the instances of Cirripedia and others in which a very evident bilateral symmetry is still in evidence. That the "cirri" of *Balanus* tend to present an axial arrangement is readily seen by an examination of the living animal from above; and that the shells and, again, the stalk of *Lepas* are axo-symmetric in their arrangement is evident.

The modification from one type of symmetry to another will be proportional in completeness to the time during which the new environment has been effective, and, in addition, to the degree of differentiation to which the organism has already attained. Thus, *Amœba* will adapt its form almost immediately to a change of environment, a Coelenterate more slowly, and a Cirripede more slowly still.

If an axis be drawn from the point of fixation of a sedentary organism at right angles to the plane of fixation, the environment on all sides of this axis will be similar (except in special cases, *e.g.*

currents of the medium) whilst the two ends of the axis will have dissimilar environments. This axis corresponds to the axis of symmetry of the axo-symmetric animals which are sedentary.

(2) *Pelagic plankton.*—If a perpendicular axis be drawn downwards from the surface of the medium of flotation, the environment on all sides of this axis will be similar whereas its two ends will be very dissimilar. Hence a floating (drifting) organism will have an axis parallel to the above and with similar properties. According to the principles of correlation, we find that the drifting plankton (Medusae, *Arcella, Pyrosoma,* etc.) are axo-symmetric about an axis which is at right angles to the surface. It may be noted that movement along this axis, *i.e.* the axis of symmetry, does not in any way affect the conditions of symmetry (Medusae, Ctenophora). It is quite irrelevant to the argument that Medusae may be descended from sedentary forms. They are axo-symmetric and pelagic drifters, agreeing thereby with an enormous number of other forms.

(3) *Free-swimming with axial rotation.*—If an organism move in one definite direction there is an evident dissimilarity in environmental conditions between the two terminal points of the axis of direction. If this direction correspond with the axis perpendicular to the surface it does not affect the axo-symmetry, for it only accentuates a dissimilarity which already existed, but if it be at any angle to the perpendicular there will be produced two axes of dissimilarity, and unless other factors intervene an axo-symmetric organism will be out of harmony with such conditions. If, however, the organism rotates about its axis of locomotion, the dissimilarity due to the physical conditions will be nullified, and the parts on all sides of the axis of direction will, in response to the similar environment, tend to become symmetrical, so that the axis of locomotive direction will be converted into the axis of symmetry and the organism will become axially symmetric.

Those who have observed the locomotion of gastrula-larvae speak of a spiral motion due to a movement forward along the main axis, combined with a rotation about the same. It is also quite possible that in many of the more active members of the pelagic plankton, in which the locomotion often brings the axis of symmetry at an angle with the perpendicular to the surface, a free capacity for axial rotation tends to perpetuate the axial symmetry.

In brief, the axo-symmetric organisms occur principally in the Protozoa, Porifera, Coelentera, and Echinoderma, and they are typically pelagic drifters, sedentary forms, or free-swimmers which rotate about the axis of locomotion, all of which habitats correspond to the theoretical conditions. In a great number of the axo-symmetric Coelentera there are two phases in the life-history (medusoid and hydroid) corresponding to the two principal conditions of existence in this type of symmetry, *i.e.* drifting and sedentary.

3. *Plano-symmetry* (Heteraxonia of some morphologists).—This

type, corresponding almost exactly with the "bilateral symmetry," is repetition of parts in only one dimension, so that the centre of symmetry is a plane formed by the two remaining dimensional axes. In this case the minimum number of secondary centres of symmetry is reduced to two, and, as there is but one dimension of symmetry, this corresponds to the predominating number.

Plano-symmetry occurs in a few of the Protozoa and Coelentera, and is found almost universally in the Metazoa above the Coelentera, though it may be more or less disguised in certain groups (*e.g.* Echinoderma, Tunicata), by a secondary superposed axo-symmetry. The environmental conditions for the production of this form of symmetry are not many. A primary physical environment fulfilling the desired conditions is not common nor easy of attainment. Locomotion of the organism in a definite direction, but forming some angle with the perpendicular to the surface or to the bottom, will supply the requisite dissimilarities of environment in two dimensions, provided that rotation about the locomotive axis be prevented. In other words, the motion is limited to one dimension, which, for the greatest effect, should be horizontal. Nevertheless, although this is the prevalent origin of plano-symmetry, a physical environment of an organism at rest is conceivable in which the necessary conditions are fulfilled. Thus fixation at one end will cause, as already shown, axo-symmetry if the axis is parallel with the perpendicular to the plane of fixation, but if they intersect at an angle, a factor of dissimilarity is produced, which, like the last, will be more potent the greater the angle. Examples of this type without locomotion in one direction are rare, but are probably exemplified in *Taenia*, *Loxosoma*, etc. In some text-books it is stated that "bilateral" symmetry is confined to animals with locomotion in one direction, but, as pointed out here, the necessary conditions are attained in a sedentary habit.

There are three possible sub-types of plano-symmetry, which are defined according to the heterogeneity in the dimensional axes. The first or tri-plano-symmetry (Fig. 4) has all three axes formed of similar component radii; and hence three planes of symmetry are present, intersecting each axis respectively. The geometrical expression of this type is the right rhombic prism or octahedron. It is included by Haeckel in a sub-group of his Centraxonia. Di-plano-symmetry (Fig. 5) has two planes of symmetry, as the radii of one axis differ. It is represented by the right rhombic pyramid. Plano-symmetry (or monoplano-symmetry) has only one plane of symmetry (Fig. 6) as the radii of two axes differ, and hence the plane of symmetry must pass through these two. It is represented by the right pyramid with a rhomboidal base. Spencer distinguishes these three as single, double, and triple bilateral symmetry (Figs. 4, 5, and 6).

The true plano-symmetry is by far the commonest, though instances of the others can be easily found. The typical Hexactinia, taking into

account the siphonoglyphs and mesenterial muscles, give a good example of di-plano-symmetry, whilst tri-plano-symmetry is found in certain Acantharia (Haeckel), and more or less perfectly in some of the Ctenophora.

There are several points to notice with regard to these three types of symmetry. These bear out our argument that they represent a natural classification.

Firstly, it has been already shown that a great number of the organisms which in other respects take their position at the bottom of

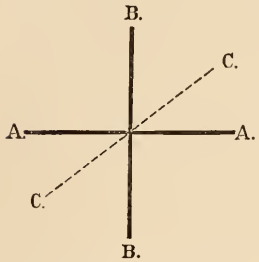


FIG. 4.—Tri-plano-symmetry. (2A + 2B + 2C.)

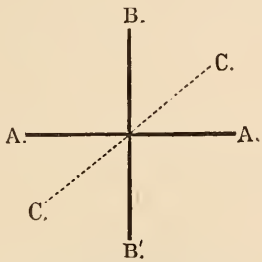


FIG. 5.—Di-plano-symmetry. (2A + B + B' + 2C.)

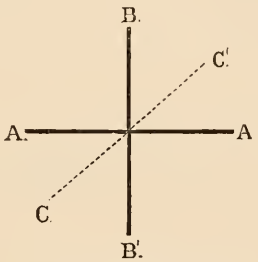


FIG. 6.—Plano-symmetry. (2A + B + B' + C + C'.)

the animal scale belong either to no type of symmetry whatever or exhibit at one stage of their career a centro-symmetry (see Fig. 9).

Again, others of the Protozoa and almost all of the Coelentera, together with other groups showing degeneration, exhibit the second or axo-symmetry ; and lastly, the great majority of the Metazoa exhibit the third or plano-symmetry more and more markedly as the highest types are reached.

This classification, therefore, clearly indicates the phyletic progress in course of time from forms with no symmetry and ever-changing shape, through symmetry in all dimensions, to that in two, and finally, the successive types of symmetry.

In this connection we may look at the increasing influence of locomotion in the determination of an animal's symmetry. The locomotion in centro-symmetric forms is quite indefinite, with freedom to rotate about the centre and nothing more. That of axo-symmetric forms is in a definite direction in a considerable number, but may be in many cases entirely absent (sedentary). At most it is definite in only one dimension. In plano-symmetric forms the vast majority have definite locomotion, with a definite direction, in two dimensions. The gradual development of symmetry from a lower to a higher type is therefore in agreement with the general facts of evolution (see Fig. 7). Similarly in ontogeny the same history is recapitulated. With exceptions mainly traceable to secondary coenogenetic modifications, the ontogeny of a plano-symmetric organism commences with the centro-symmetric egg, which in the most primitive forms retains its centro-symmetry up to the free swimming centro-symmetric blastula-larva. The

presence of yolk may, in certain cases, enable the embryo to, as it were, take a short cut to axo-symmetry (unequal segmentation), otherwise the blastula in due course becomes the gastrula, a typical axo-symmetric organism, with locomotion in one direction and rotation about the axis so defined. Whilst the lower forms (Coelentera) remain at this stage, the higher pass on to the type of plano-symmetry, and thus the phyletic history is repeated (Fig. 7).

Reference has been made above to the secondary centres of symmetry and the predominant number of these in the three types of symmetry was stated to be six, four, and two respectively. Any organs in an organism must be centric or peripheral. If they are in the former

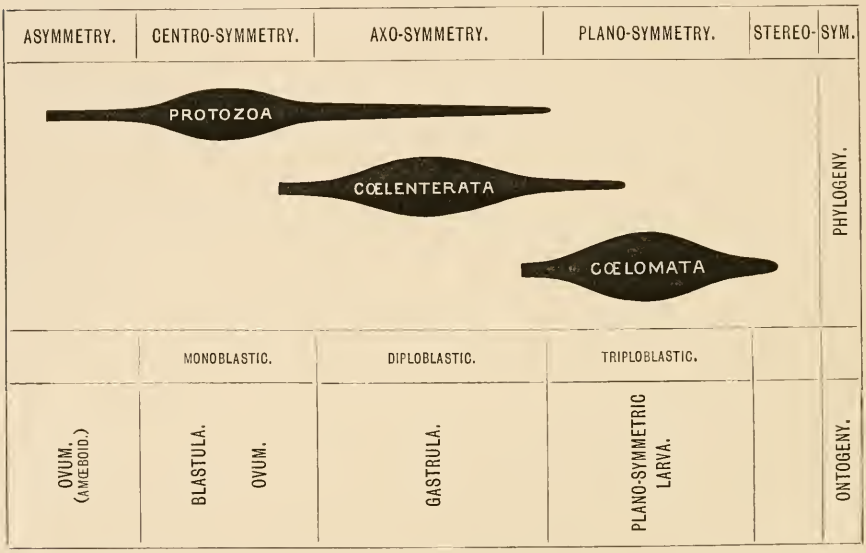


FIG. 7.—Diagram to indicate the relative predominance of each main type of symmetry in the chief divisions of the animal kingdom, and the gradual advance of type both in phylogeny and ontogeny.

category and conform to the symmetry of the organism, they must be single, and must be conformable to the centre of symmetry, whether a point, an axis, or a plane. If they are peripheral they must, in order to conform to the symmetry of the whole organism, be repeated a certain number of times round secondary centres of symmetry, and the primary numbers corresponding in each case to twice the number of symmetrical dimensions, work out as six, four, and two.

This subject is intimately connected with the theory of "metameric segmentation" and its attendant phenomenon and will be more fully dealt with later.

4. *Sterco-Symmetry.*—We have discussed in turn the three forms of symmetry, and the question remains—Is there not yet another? Our definition of the symmetry of an organism reads as follows:—The system of arrangement of its constituent parts in relation to each other and to a

certain geometrical centre. In the former three systems the units were arranged with respect to (1) a point, (2) a straight line, (3) a plane. Each of these two last geometrical expressions is formed by the motion of the former, thus conferring upon the centre of symmetry an additional dimension, and lastly, the motion of the third centre of symmetry, or the plane, results in a centre of symmetry of three dimensions. In the same way, in centro-symmetry we found that organs with secondary centres were repeated in three dimensions (6), those in axo-symmetry in two dimensions (4), and those in plano-symmetry were repeated in one dimension (2). Similarly in this last type (stereo-symmetry), as the centre of symmetry is tri-dimensional, the organs will not be repeated at all (Fig. 8). This is evidently an entirely different condition from asymmetry, in which there is no "system of arrangement of the parts" found in the amorphic Protozoa and others (Fig. 9).

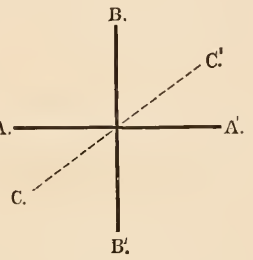

FIG. 8.—Stereo-symmetry. (A + A' + B + B' + C + C'.)

In the first three forms of symmetry one can notice how the organism, by its gradual differentiation, becomes as it were the determining factor in its relations to the environment. Thus it is mainly the mode of locomotion which is adopted (or the introduction of voluntary movement) that results in the production of a plano-symmetric organism, and in a precisely analogous manner in stereo-symmetry the organism becomes more independent and predominant over its own fate in determining the way in which its environment shall affect it.

The gradual evolution in the animal kingdom results in a graduated succession of organisms becoming more and more independent of their environment, in so far as that environment has power to mould the organismal form into harmony with itself.

In the preceding cases the organisms exhibiting those types of symmetry are to that extent in equilibrium with the same type of environment, and in asymmetry the organism rapidly changes its form to adapt itself to a change of environment, but in a stereo-symmetric form the organism has so far brought itself to be partially equilibrated to any change of environment without actual change in general form on its own part, that it may be said practically to have released itself from its environmental thraldom, as far as symmetry is concerned. Hence its organs will never be repeated in cases where the pair are not absolutely inter-dependent (*e.g.* the paired eyes), and the form of the organism will be determined solely by the inter-relations of its parts. This ideal is not as yet reached in the animal kingdom, for all the plano-symmetric animals still show marked traces of their "paired" condition. On the other hand, stereo-symmetry is continually being evolved in them by a division of labour between the two "paired" elements, so that in the highest types the "bilateral" or paired arrangement is to some extent effaced.

Perhaps the most recent example of this type, in the highest of animals, is the universal tendency to right or left-handedness in man.

This fourth type of symmetry must be carefully distinguished from the cases of peculiar arrangement of parts due to the superposition of one type of symmetry upon another, or even of one type upon itself but in a different relation to the organism. Thus, in the majority of the Echinoderma the "plane" type has probably been impressed upon the "axial" of the Coelentera, only to be in its turn "covered" by the axial type of the sedentary period. Lastly, some species, such as *Spatangus,* have early traces of a "plane" type again superposed upon the "axial," and a further reversion to the "axial" is found in *Pelagothuria.* Again, in the Pleuronectidæ the so-called asymmetry is evidently due to a second plano-symmetry superposed upon the first, with the two planes of symmetry perpendicular to each other. In all these cases the transition will involve an apparent asymmetry or stereo-symmetry.

The four types of symmetry and their sub-types may be shown diagrammatically as in Figs. 1-6, and Fig. 8, in which each of the three dimensional axes is indicated, and their resemblances or differences are shown by letters. The increasing heterogeneity as the various types are passed through is shown clearly by the formulae below the figures. Again, if a regular tetrahedron be taken instead of an octagon, because it is the simplest possible solid figure, then the four main types of symmetry are indicated as in Figs. 9-12, whilst in this case the sub-types can only be shown by a reference to the planes of symmetry of a tetrahedron, into which it is not necessary to enter.

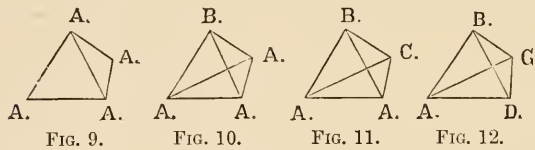


Fig. 9. Fig. 10. Fig. 11. Fig. 12.

Fig. 9.—Centro-symmetry. (4A.)
Fig. 10.—Axo-symmetry. (3A + B.)
Fig. 11.—Plano-symmetry. (2A + B + C.)
Fig. 12.—Stereo-symmetry. (A + B + C + D.)

The term "asymmetry" should be strictly confined to the absence of all definite "arrangement," and in this sense it has been used by Spencer.

The "asymmetry" of the chemists and physicists is, however, exactly analogous to stereo-symmetry as here defined, and both phenomena are alike due to the highest possible "arrangement" of the mass in each case. Pasteur's conclusions with regard to the intimate connection of molecular stereo-symmetry and organic phenomena have been recently brought forward with renewed emphasis by Professor Japp.[1] The ascertained fact that the molecular stereo-symmetry is a characteristic feature of "organised" matter, pointed out by him, is interesting in view of the fact that the evolution of symmetry in the animal kingdom indicates a gradual transference of stereo-symmetry from the molecule

[1] President's Address, Chemical Section, British Association, Bristol 1898.

to the entire individual, although the latter end of the series has not yet been completely attained. There is also this parallel, that in the formation of the stereo-symmetric individual only one "form" is evolved, just as occurs in vital molecular phenomena in contradistinction to purely chemical.

Thus we do not find any human body with a liver on the left side, with stomach and spleen on the right, a right aortic arch and a left azygos vein, and so on, though it would be hard for an anatomist to pronounce that such an enantiomorph of the human subject is an impossibility, and harder still to say why such is not in existence.

Could an experimental morphologist immensely hasten the processes of evolution there should be no difficulty in subjecting any given organism to a special environment, and, by the inter-action of the two, producing an organism exhibiting any of the three types of centro-, axo- or plano-symmetry. By having recourse to a special environment (separation of the blastomeres) he may even to some extent produce stereo-symmetric organisms, but in equal quantities of right and left-sided individuals, unless one enantiomorph is destroyed purposely in the making. By no process could he produce a single stereo-symmetric "form" without its enantiomorph, because the origin of such lies in the molecular constitution of the organism and has no connection with the environment.

Though not expressed in chemical language, the analogy of the above to molecular stereo-symmetry is complete.

The asymmetric lowest type of organism changes its form continually and is the "sport" of its environment; the centro-symmetric, axo-symmetric, and plano-symmetric form gradations in which the organism becomes more definite and permanent in shape, as it becomes less dependent on its environment and more voluntary in its actions. Lastly, a purely stereo-symmetric organism (with a stereo-symmetric ontogeny), the possible product of future evolution, will have so far freed itself from the effect of its environment that changes in the latter will no more affect its morphological plan (complete heterogeneity). The homogeneity of structure will then have passed from the three dimensions of space to the so-called "fourth dimension" of time.

The study of animal symmetry points to no conclusion more clearly than the future development and predominance of the stereo-symmetric individual, in which the fundamental stereo-symmetry of the physical unit has extended itself to the morphological stereo-symmetry of the organism.

The University,
St. Andrews.

# FRESH FACTS.

A New Amoeba in Man. J. Ijima.—"On a new Rhizopod Parasite of Man—*Amoeba miuria*, n. sp." (*Annot. Zool. Japon.* ii. (1898), pp. 85-94). This parasite was found in abundance in the serous fluid-accumulation of the peritoneal and pleural cavities in a case of peritonitis and pleuritis endotheliomatosa, but probably had its headquarters in the tumour tissue. Its nearest allies seem to be *A. villosa*, Wallich, and *A. fluida*, Gruber.

Respiratory Trees of Holothuroids. L. Bordas. — "Anatomie et fonctions physiologiques des organes arborescents ou poumons aquatiques de quelques Holothuries" (*C. R. Ac. Sci.* cxxvii. (1898), pp. 568-570). At the marine laboratory of Endoume (Marseille), Bordas made a number of observations and experiments on *Holothuria impatiens*, Gmelin, *H. Poli*, Delle Chiaje, *H. tubulosa*, Gmelin, and *Stichopus regalis*, Selenka, and succeeded in showing that the arborescent organs, which develop as diverticula of the gut, have at least four important functions. They are respiratory, as most zoologists have recognised; they have a hydrostatic function when the body expands; they produce numerous amoebocytes; and they are excretory, as is shown by the presence of uric acid and urates.

Mud from Jerusalem. E. Atkinson.—"Extraordinary Vitality of Entomostraca in Mud from Jerusalem" (*Ann. Nat. Hist.* ii. (1898), pp. 372-376). Forty years ago Mr. Atkinson took some samples of mud from the ancient pool of Gihon, outside the Jaffa Gate of Jerusalem, which at that time contained water during only two months of the year. The dry mud was sent to England and moistened, with the result that six new species of living Entomostraca were detected by Dr. Baird. For eight years in succession, at the Leeds Philosophical Society's Museum, the mud was dried up in summer and moistened again in spring, and its tenants still persisted. In one case a small sample was left dry in a pill box for nine years, and then moistened, with the result that in a fortnight a single specimen of *Estheria gihoni* made its appearance. In another case, the alternation of drought and moisture was kept up artificially for twenty-four years, with unvarying success as regarded persistence of vitality.

Grafting Insects. Henry E. Crampton, Jun.—"An Important instance of Insect Coalescence" (*Ann. New York Ac. Sci.* xi. (1898), pp. 219-223). In about twenty cases out of nearly two hundred experiments, Mr. Crampton succeeded in effecting a grafting or coalescence of lepidopterous pupae, similar to that obtained by Born with the embryos of Amphibia. He has proceeded to inquire whether in such coalescence the colours of one moth could be made to replace those of another by a transfusion of haemolymph. In a series of over 750 experiments one case of exceptional interest occurred. A pupa of *Callosamia promethea* was united "in tandem" anteriorly to a pupa of *Samia cecropia*, the

two being kept together by means of melted paraffine applied to the edge of the common wound. The result, as regards coloration, was that while the *cecropia* exhibited, as far as could be determined, the normal specific colours, portions of the *promethea* wings presented the colours characteristic of only the wings of *cecropia*.

Mosquitoes and Malaria. B. Grassi.—"La malaria propagata per mezzo di peculiari insetti" (*Atti R. Accad. Lincei* (*Rend.*) vii. (1898) pp. 234-240). Prof. Grassi has published a second preliminary note in support of his important theory that various "mosquitoes" (*Anopheles claviger*, *Culex penicillaris*, and *Culex malariae*) are agents in spreading malaria. He takes account of other workers who have suggested this.

Pearl-Making. Louis Boutan.—"Production artificielle des perles chez les Haliotis" (*C. R. Ac. Sci.* cxxvii. (1898) pp. 828-830). By "trepanning" the shell and introducing minute spherules of nacre, Boutan was able to induce the formation of "really fine pearls," which cannot be called false.

Pigmentation of the Mussel. Victor Faussek.—"Ueber die Ablagerung des Pigmentes bei Mytilus" (*Z. wiss. Zool.* lxv. (1898) pp. 112-142). A number of experiments show that the formation and deposition of pigment in the mantle, gills, and other parts of the common mussel is not at all affected by alterations in the light, but is in part regulated by the variable degree of exposure to oxygenated water, taken in connection with the distribution of the blood-vessels.

Respiration of the Lamprey. E. Couvreur.—"Étude sur la respiration des poissons. Mécanisme respiratoire chez les Cyclostomes" (*Ann. Soc. Linn. Lyon*, xliv. (1898) pp. 105-109, 2 figs.) In the young lamprey, in the Ammocoete stage, the respiratory movements occur about eighty times a minute, mounting up to a hundred during excitement, but are occasionally suspended for a considerable period. During inspiration the lingual piston is projected, the body-walls expand, the spiracula (external apertures) are opened by the relaxation of the sphincters, and the oscula (apertures into the respiratory tube) are opened by the forward movement of the piston. The water then enters the branchial sacs by the spiracles and oscula simultaneously. During expiration the piston retracts, the spiracles are slightly open, the oscula are closed. The water then passes out by the spiracula. When the lamprey is fixed by its mouth it expires and inspires only by the spiracula, as Duméril observed long ago. Couvreur adds the interesting note that the retraction of the piston corresponds to the systole, and its projection to the diastole of the heart.

Syncytia in Development. W. His.—"Ueber Zellen- und Syncytienbildung. Studien am Salmonidenkeim" (*Abh. K. Sächs. Ges. Wiss.* xxiv. (1898) pp. 401-468, 41 figs.) In this important and beautifully illustrated paper, Prof. His discusses syncytia, *i.e.* "complexes of mutually connected histological units or plasmochores, which are distinctly separated from one another by limiting areas or diastemas." Their primary origin is from incomplete processes of division, but they may also arise secondarily by the marginal coalescence of previously distinct cells. Both types occur abundantly in the blastoderms of fishes. The paper ends with the weighty statement that syncytia and pluripolar nuclear divisions, and giant-nuclei or syncaryoses, are associated phenomena, always implying the occurrence of intense plasmic activity and favourable nutritive conditions.

ONE IN THE EYE. FRANK FINN.—"Note on the Long-Snouted Whip-Snake" (*Dryophis mycterizans*) (*J. Asiat. Soc. Bengal*, lxvii. (1898) pp. 66-67). Mr. Finn had two of these (harmless?) snakes in his hands, and was holding them gently, when the larger one, which had previously struck at his hand, made a sudden dart at his eye. As he instinctively closed the threatened organ, the snake only succeeded in making two small bites on the upper eyelid and one on the lower. It left one of its teeth, rather over $\frac{1}{20}$ inch in length, but this proved not to be a grooved one. No inconvenience was felt. Mr. Finn thus unexpectedly verified what is a common belief in India that the whip-snake strikes deliberately at the eye, an interesting trait which, as the author notes, is not alluded to by either Dr. Günther or Mr. Boulenger in their accounts of the Indian Reptilia.

A PROBLEM OF SEX. R. W. SHUFELDT.—"On the Alternation of Sex in a Brood of Young Sparrow-Hawks" (*Amer. Nat.* xxxii. (1898) pp. 567-570, 1 fig.) In a brood of *Falco sparverius* Dr. Shufeldt found that the oldest of the five was a male, the next a female, and so on in regular alternation. He inquires whether the alternation is the rule, and, if so, what the correct interpretation of it can be.

THE KANGAROO'S VOCAL CORDS. JOHNSON SYMINGTON.—"The Marsupial Larynx" (*Journ. Anat. Physiol.* xxxiii. (1898) pp. 31-49, 8 figs.) From among the many interesting facts stated in this paper we select one—that the vocal cords of pouch specimens of *Macropus bennettii* are well developed, while the adult vocal cord must be regarded as a degenerated structure. "This is interesting in connection with the well-known fact that these animals are voiceless, and it suggests the theory that they are descended from a stock which possessed a voice."

# SOME NEW BOOKS.

## BARK-BEETLES

De Danske Barkbiller (Scolytidae et Platypodidae Danicae). By E. A. Løvendal. 4to, pp. xii. + 212 with figs. and 5 pls. Copenhagen, 1898.

From the earliest period of modern natural history research the Bark-beetles have attracted much attention. Their importance as widespread destroyers of trees (rarely of other plants, as in the case of *Xyleborus morigerus*, an exotic species now only too common among Dendrobiums in European hot-houses), and the singularity of their burrows in bark or wood have given rise to a literature, beginning with the last century tracts on the "Wurmtrockniss" or destruction inflicted on conifers by various species of *Ips* (*Tomicus*), which is of surprising extent, and with which, concealed as much of it is in obscure periodicals on Forestry, etc., it is difficult to obtain a competent acquaintance. For all but historical purposes, however, it may be said to begin with Ratzeburg's "Forst-Insekten," a work which laid the foundation for the scientific study of forest entomology.

With entomologists not concerned with economic work the study of this family has not proved popular, and it is only in the last thirty years or so that anything has been known about extra-European species. This is not surprising, for the insects are small and obscure, and often so alike in appearance, that they cannot be satisfactorily determined except after prolonged study, not only of the simpler external features, but of the mouth-parts, antennae, and legs, a task requiring much tedious and exacting microscopical work.

They are nevertheless well deserving of study by every one who is prepared not to measure the importance of systematic work by the showiness of the collections on which he is engaged. Accurate determination is the first necessity in the case of many economic problems presented by these beetles; indeed, without it divers of them are incapable of solution. And even if the European species present no great variety of structure, the exotic forms sometimes exhibit a singularity that is scarcely exceeded by any family of Coleoptera.

The habits of the ordinary bark-burrowing Scolytids are peculiar among beetles; still more strange are those of the wood-borers, who, though they tunnel, do not feed on wood but on fungus growths, which, as Habbard has lately shown, are artificially propagated by the insects themselves.

In *Xyleborus*, one of the best known of the wood-boring and "ambrosia-feeding" genera, the males are sub-apterous, dwarfed, and often of bizarre form (in *X. morigerus* the male is relatively so minute that it might be mistaken for an insect of a different genus or even family from the female). Moreover, they are very much the rarer sex, their numbers varying from one in four to one in fifty, or thereabouts; the species are therefore polygamous, and must in most cases inbreed, a fact which does not prevent the genus from being one of the richest and most widely-distributed of the Scolytidae.

One other peculiarity of the family may be selected for mention. In about a half of it, certain deviations from the normal type of structure, such as great antennal development, excavation of the forehead or its decoration with hair-tufts, are, so far as is known, diagnostic of the males; in the other half precisely similar characters when present occur in the females. The line of demarcation between these halves appears to be arbitrary, and this counter-change of sexual characters in different portions of the same family has given rise to some little confusion, and is not the least interesting of the many biological problems provided by the Bark-beetles.

To the late Wilhelm Eichhoff, more than to any one else, is due the present state of our knowledge of these insects. By profession a forester, he supplemented his necessary familiarity with their economy by careful systematic study, and was the first to make a thorough investigation of their oral and antennal structure, and, together with Chapuis, to compile really competent descriptions of exotic species. Eichhoff's *Ratio, &c., Tomicinorum*, published in 1879, is a model of accurate diagnosis, and it is surprising with what ease and certainty the species of the very difficult Tomicinæ are to be identified thereby.

Two years later Eichhoff published "Die Europäischen Borkenkäfer," an account written on less technical lines of the habits, economy, and characters of the European species, and indispensable to the worker at forest entomology.

In the handsome quarto before us Mr. Løvendal, of the Copenhagen Museum, has done on a more extensive scale for the bark-beetles of Denmark what Eichhoff did for those of Europe; and we can but regret that the utility of the book is restricted by the comparatively little known language in which it is entirely written. Mr. Løvendal has aimed at exhaustiveness of treatment both in his descriptions of the insects, at least so far as their external characters are concerned (for he does not follow Lindeman of Moscow far in that writer's elaborate and not very productive researches on internal anatomy), and in his accounts of their life-histories and economic relations. So far as we can judge, he has fully succeeded; and the labour devoted to the book may be estimated by the fact that the list of works cited numbers close on two hundred, in which we have not detected any important omission.

The book, like that of Eichhoff, is furnished with full, perhaps even unduly elaborate, tables of generic and specific characters, and for identification of the species by means of their galleries and the kinds of tree attacked. In its plan it closely follows its predecessor, so that those who know that useful work will know what to expect here, though they will probably be surprised at the scale on which it is carried out.

Not its least valuable part is the full information given as to distribution both within and without Denmark, the author having consulted all papers throwing any light on this subject. We notice however, that, though he records the existence of *X. saxeseni*, Ratz., in North America, he, like most European entomologists, is unaware that it was first described there, and should bear the name of *X. xylographus*, Say.

The number of species recorded from Denmark is fifty-one; this is about equal to the number in the British Isles, about a dozen species in each fauna not being found in the other. It is likely that further research will increase the number known from Denmark, as several species not recorded by Løvendal may be expected to occur there. One species only is peculiar, *Ips elongatus*, Løv.; this has been treated by Reitter as synonymous with *Ips austriacus*, Wachtl., an identification which Løvendal does not accept.

The text is illustrated with numerous pictures of burrows in bark and wood, partly original, and partly copies. The galleries of several species are, we believe, illustrated for the first time.

But perhaps the most striking feature of the whole work is that of the five plates, drawn and engraved by the author in a manner now rarely to be seen in

entomological works, but familiar to those who know Løvendal's plates to his countryman Schiødte's "De Metamorphosi Eleutheratorum." No more beautiful or accurate illustrations of these insects, so hard to delineate successfully, have ever been made, and they alone entitle the book to distinction.

To write a discriminating notice of a work in a language that one can read but slowly and laboriously is impossible. But, so far as we have been able to form an opinion, we cannot say less than that we regard this work as one of the most thorough and perfect faunistic monographs it has ever been our good fortune to examine. It furnishes the Danish entomologist with everything he can require to know on these interesting and important little beetles. It may be added that its publication has been rendered possible by a State subsidy from the Carlsberg Fund. W. F. H. Blandford.

## ANOTHER TEXT-BOOK OF BOTANY.

An Elementary Text-Book of Botany. By Sydney H. Vines, M.A., D.Sc., F.R.S. 8vo, pp. xv. and 611, with 397 illustrations. Sonnenschein and Co., London, 1898. Price 9s.

Those of us who remember the old "Prantl and Vines" which, fifteen years ago, was perhaps the most generally used elementary text-book in medical schools and colleges, will scarcely recognise any resemblance to it in the present volume. This is not a new edition of the old translation from the German of Prantl, but a new text-book brought almost as nearly up to date as is possible for an elementary work. It vividly recalls the Students' Text-Book of 1894-95, and is, so to say, a carefully edited abridgment of the latter. Difficult and debatable topics have been omitted, the more fundamental recent discoveries have been incorporated, and there has also been some rearrangement of the subject matter. The result is a diminution in bulk by one quarter, and a presentation of the elementary facts of botany in so complete a manner as to place the Students' Text-Book at 16s. in the position merely of an *édition de luxe*. In fact, the author has created the necessity for the more advanced and larger edition of the latter which he contemplates in his preface, and which we shall hope to review at no very distant period.

As *Natural Science* has already noticed at some length the Students' Text-Book (see vol. iv. p. 376, and vol. vi. p. 424), and the present work is on the same lines, it will be sufficient to note here the main points of difference between the two. Modern tendency is to bring as nearly together as possible the stories of form and function. Van Tieghem, in his most recent text-book on Botany, has incorporated them in one section, and discusses *seriatim* the morphology and physiology of the various plant-members. This mode of treatment, though doubtless the most scientific, has many disadvantages. Professor Vines does not attempt it, but we are glad to see that the section on Physiology is brought from the end of the book, and follows that on Morphology and Histology. These first three parts, dealing respectively with general form, anatomy and histology, and physiology, leave little to be desired. The confusion over the Stelar theory, which has bothered so many students in the larger work, is here avoided, doubtful points relating to the minute structure of the cell are omitted, and a tangible physical explanation is given of the ascent of the sap, based on the recent work of Messrs. Dixon and Joly. In Part IV. (Classification) we are a little disappointed to find no revision of the sub-classes Algae and Fungi, and in Group II., Bryophyta, we should like to have seen a fuller description of at least one typical thalloid and foliose member of the class Hepaticae.

In the treatment of the Seed-plants, account is taken of the recent discovery of motile male cells in the Cycads and Conifers, and two distinct groups, each comparable with Bryophyta or Pteridophyta, are now recognised under the old class-names Gymnospermae and Angiospermae. The general introduction to

these groups is excellent, but the arrangement of the sub-classes and cohorts of the Monocotyledons and Dicotyledons is open to criticism. The author has "thought it desirable to follow, in the main, the classification laid down in the *Genera Plantarum* of Bentham and Hooker," one of the classics of Systematic Botany, but a work which occupied half a lifetime, and was completed fifteen years ago. As in Morphology and Physiology, so in the study of the affinities of the families of seed-plants, much has been done in the interval, and some expression of this should find place in an elementary text-book.

## THE COMPLETE GENEALOGIST.

Lehrbuch der gesammten wissenschaftlichen Genealogie. Stammbaum und Ahnentafel in ihrer geschichtlichen, sociologischen und naturwissenschaftlichen Bedeutung. By Dr. Ottokar Lorenz, Professor der Geschichte. 8vo, pp. ix. and 489. Berlin: Hertz, 1898.

More than a hundred years ago, as Dr. Lorenz tells us, Gatterer of Göttingen wrote a text-book of genealogy for the use of his students. And, strangely enough, this old book has remained until now the only systematic treatise on the subject. It was high time that there should be a new "Gatterer," and this our author has supplied, to the gratification of a wide circle of students, whether of history, or heraldry, or heredity. Dr. Lorenz discusses "genealogy as a science," "the theory of the genealogical tree," "tables of ancestry," and "the bearing of genealogical studies on heredity." He deals very judiciously with modern theories of heredity, inclining on the whole to agree with Weismann; and makes a strong case for the advantage of genealogical studies to supplement those which are purely statistical. In our judgment he does not fully appreciate the importance of Mr. Galton's work, especially as regards "the law of ancestral inheritance." X.

## THE GEOLOGIST'S VADEMECUM.

Aids in Practical Geology. By Grenville A. J. Cole. Third edition. 8vo, pp. xvi. and 432, with coloured frontispiece and numerous illustrations. London: C. Griffin and Co., 1898. Price 10s. 6d.

Good wine needs no bush, and Professor Cole's excellent adjunct to the text-book and the laboratory, having now reached its third edition, requires little further commendation from us. The first edition was published in December 1890; the second in April 1893. The present edition is enlarged by several pages, and is revised throughout. New inventions of apparatus are introduced in the text, and the more recent literature referred to in the footnotes. A good deal of improvement and extension is visible in the palaeontological part, which is better than most English text-books of equal size. Nevertheless we are not sure that we understand the reason for this section. It would not really be of much use in the field; and it is certainly not a guide to laboratory practice or to original investigation. Still we have little fault to find with it, except that the true scale of the various figures is not indicated. On the whole we should recommend Professor Cole to reduce those portions of his book that cover ground which is, or ought to be, covered by the ordinary text-book, and to amplify the strictly practical chapters, which already are admirable.

Dr. John Anderson has completed his researches on the Reptilia and Batrachia of Egypt, and the results form volume one of the *Zoology of Egypt*, which has just been announced by Bernard Quaritch. Only 100 copies have been printed, and the price is £12 : 12s., which is quite prohibitive. Some 1500 specimens were collected, the greater part of which are now in the British Museum.

Students of the Foraminifera will be glad to learn that the *Société de Naturalistes de Kiew* (Kieff), Russia, have decided to publish a continuation of Sherborn's *Bibliography of the Foraminifera*, from 1888-98. The compilation has been undertaken by Dr. Paul Tutkovski, whose work on the Russian tertiary and cretaceous Foraminifera is well known, and who has been in frequent communication with Mr. Sherborn and other workers. We may therefore look forward to a full and complete additional list which will be of much service and value.

The fifth year of the *Bibliography and Index of North American Geology, Palaeontology, Petrology, and Mineralogy*, that for 1897, has just appeared. It forms the 156th *Bulletin of the United States Geological Survey* and is compiled by Fred Broughton Weeks. There are 742 separate titles which are placed in alphabetical order under authors, and these titles are analysed in 40 pages of index. The whole forms a compact and handy Bibliography in an inexpensive and practically useful form.

*Science Progress: a Monthly Review of current Scientific Investigation*, changed some time ago into a quarterly, and now, to the considerable loss of its writers and readers, disappears from the scene. Instead, there enters *Science Work: a Monthly Review of Scientific Literature*, published by Robert Aikman and Company, Manchester, and edited by Waller Jeffs. This is to afford a practical guide to publications in the English language on all branches of natural and social science. The subscription is three shillings a year; if prepaid, half-a-crown, post free. It will doubtless prove useful to those who wish to know what is appearing in the magazines. May we express a hope that our next fellow-worker in this field will choose a title less reminiscent of *Natural Science: a Monthly Review of Scientific Progress?*

Mr. Wilfrid Mark Webb has given up the editorship of the *Journal of Malacology*, as well as his position under the Essex County Council, and has removed from Brentwood to 2 The Broadway, Hammersmith. He is temporarily engaged on work at the museum of Eton College. *The Journal of Malacology* passes again into the hands of Mr. W. E. Collinge and also becomes in part the organ of the Midland Malacological Society, which was founded on July 7, 1898, with Mr. Collinge as president, and Mr. H. Howard Bloomer as secretary. The first number of vol. vii. of the *Journal* was issued on December 2. It contains "Descriptions of a New Species of *Cryptosoma* (*C. austeni*)," by W. E. Collinge; a reprint of the description, with figures, of "Species of *Plectopylis* recently described in *Science Gossip*," which does not give the dates of the original publication, an unfortunate omission considering the vagaries that have from time to time affected the issue of our estimable contemporary. The Bibliography of current Malacological literature is now restricted "to books and papers sent in by their respective authors, and those of special interest in the current magazines, etc." There is much virtue in an "etc."

We have received Volume xi. of the Memorias y Revista de la Sociedad Cientifica Antonio Alzate, including a new theory of respiration, by Prof. A. L. Herrera and Dr. D. Vergara Lope; a description of the volcanoes Colima and Ceboruco, by E. Ordóñez; a case of pulmonary tuberculosis cured by the action of rarefied air, by D. Vergara Lope; Metamorphoses of *Papilio daunus* and note on the staminal dimorphism of *Solanum cornutum*, with a plate, by L. G. Seurat; seismic observations made at Orizaba in 1895, by C. Mottl; albinism in the squirrel, with plate, by A. Dugès; the origin of individuals: the construction of the organism through internal conditions, by A. L. Herrera; seismic study of Central and South America, by F. de Montessus de Ballore.

Mr. F. H. Perry-Coste publishes in the December number of *Nature Notes* an interesting account of Philip Miller, "Gardener to the Worshipful Company

of Apothecaries at their Botanical Garden at Chelsea, and F.R.S.," and of his views on the circulation and nature of sap, as published in the second edition of his "Gardener's Dictionary," 1733.

The Society Carlos Ribeiro at Porto, Portugal, has changed the title of its organ from *Revista de Sciencias Naturaes e Sociaes* to *Portugalia: Materiaes para o Estudo do Povo Portuguez*. It will be devoted entirely to the anthropology and ethnography of the Portuguese race.

*Véstnik slovanskych starožytnosti* means "Review of Works on Slavonic Antiquities," and is a journal of which the first part has just been published at Prague, under the editorship of Prof. L. Niederle; most of the reviews are in French and German.

Mr. G. W. Bulman contributes to the December number of the *Westminster Review* a criticism of the late Professor G. H. T. Eimer's theory of organic evolution.

The *American Anthropologist* is to be succeeded by a new Journal, in quarterly numbers of about 200 octavo pages, published by G. P. Putnam's Sons, and conducted by the following editorial board:—F. Baker, F. Boas, D. G. Brinton, G. M. Dawson, G. A. Dorsey, W. H. Holmes, J. W. Powell, F. W. Putnam, with F. W. Hodge as secretary and managing editor. The first number is to appear in January. The annual subscription is $4.

No. 137 of vol. xviii. of *Johns Hopkins University Circulars* is devoted to "Notes from the Biological Laboratory," edited by Prof. W. K. Brooks, and "Notes from the Geological Laboratory," edited by Prof. W. B. Clark. The contents include the following papers:—"Some Ectosarcal Phenomena in the Eggs of *Hydra*," by E. A. Andrews, dealing with filose and amoeboid activities. "The Echinoids and Asteroids of Jamaica," a systematic list, with general notes, by H. L. Clark; no new species are described, but if we are to judge from the mode of printing the names, all the species are referred to genera other than those in which they were placed by the original describers. "Embryology of *Ophiocoma echinata*, Agassiz," by C. Grave; the abundance of apparently normal larvae having two anterior coelomic sacs, each communicating with the exterior by a dorsal pore-canal, is a point of much interest. "Notes on the Ophiurids collected in Jamaica during June and July 1897," by C. Grave; this resembles Dr. Clark's list, but all the species appear to be left in their original genera. E. W. Berger abstracts the late "Dr. F. S. Conant's [MS.] Notes on the Physiology of the Medusae." L. E. Griffin publishes "Notes on the Tentacles of *Nautilus pompilius*," preliminary to a complete account of the anatomy of the nautilus. The chief article in the geological section is entitled "An Episode during the Terrace Cutting of the Potomac," by Cleveland Abbe, jun. The price of this number is 10 cents, post free.

A Catalogue and price-list of the papers of the late Professor Edward D. Cope, arranged chronologically, also price-list of Plaster Casts, has been sent to us by Mrs. E. D. Cope, Haverford, Pa., U.S.A. The papers are to be had for very moderate prices, and the complete set costs seventy-five dollars. The casts may be ordered white, or carefully coloured after the originals.

The list of papers is a useful bibliography, but it is naturally far from complete, and it is marred by an excessive number of reprints. A writer so voluminous as Cope, making his important contributions to knowledge in a multitude of small scattered papers and notes, deserves a special bibliography prepared by an expert. We commend the suggestion to the notice of the Smithsonian Institution, which has already issued several valuable bibliographies of this kind.

# OBITUARIES.

## WILLIAM GUYBON ATHERSTONE.

Born 1813; Died 26th June 1898.

The Hon. William Guybon Atherstone, M.D., F.R.C.S., F.G.S., and Hon. Vice-President of the Geological Society of South Africa, died in the 85th year of his age, at his residence in Beaufort Street, Grahamstown, Cape Colony. In 1839, after settling in medical practice at Grahamstown, Dr. Atherstone met Staff-Surgeon Jameson, who was studying local geology, and he became interested in the science. A year or two afterwards Mr. A. G. Bain brought his wonderful reptilian fossils to Grahamstown, and Dr. Atherstone took up the subject earnestly. Bain states, in a lecture at that city in 1856 (reprinted in the *Trans. Geol. Soc. S. Africa*, vol. ii. part 5, 1897, p. 66)—"He said he was now determined to study geology, and should begin forthwith; and well has he kept his word, for I never met with one who made such astonishing progress in such a short time. From that day an intimacy began between us, which soon ripened into a friendship which, I trust, may never cease while we live." He goes on to say—"My friend Bochards [of Fort Beaufort] and myself had now added to our ranks the transcendent talents of Dr. Atherstone, which soon imbued our minds with elevated ideas, and gave fresh vigour and stimulus to our pursuits."

Grahamstown also supplied, in Mr. W. Ogilvie, another friendly sympathiser for Mr. Bain; and indeed this city has been termed the birthplace of South African geology; for the hearty recognition given to Mr. Bain's geological researches by the Geological Society of London stimulated others to collect fossils and examine sections throughout the country. Thus one geological society was started at Grahamstown by Dr. Atherstone, and one at Graaf-Reynet by Dr. R. N. Rubidge (afterwards of Port Elizabeth). For many years, both in the prime of life and in old age, when afflicted with blindness, Dr. Atherstone not only willingly, but enthusiastically, gave all the help he could to the establishment and well-being of the Albany Natural History Museum and the Public Gardens in Grahamstown.

Soon after their first meeting, Atherstone joined Bain in a geological visit to the Gamtoos, Bushman, Sunday, and Zwartkop Rivers, where Jurassic and Tertiary fossils were found in plenty. Interesting notes of the journey appeared in the *Eastern Province Monthly Magazine*, and the fossils have been described in the *Transactions* and *Journal Geol. Soc. London*. Further, a systematic account of the Uitenhage series of strata and their fossils in the district traversed by the above-mentioned rivers was published in the *Quart. Journ. Geol. Soc.* vol. xxiii. (1867), pages 149-171. He became a Fellow of the Geological Society of London in 1864.

Atherstone and Rubidge visited Namaqualand to examine the copper-bearing rocks, and reported on them in 1857. The Kasonga and the Alum Caves in the Eastern Province were also visited by Atherstone in or about 1858. In

1875 he came to England, having been officially requested to gather information about lunatic asylums, in view of the erection of a new one in the Colony.

With reference to his many presentations of good reptilian fossils to the British Museum, a well-deserved tribute of praise has been given to Dr. Atherstone by Sir Richard Owen in his *Descript. and Illustr. Catal. Foss. Reptilia S. Africa*, 1876, p. vi., where he says: "It will be seen how largely Science and the British Museum are indebted to W. Guybon Atherstone, Esq., M.D., who has devoted the leisure of a long and successful medical practice at the Cape to the study and acquisition of evidences of the palaeontology of that part of the globe."

Through Dr. Atherstone's intelligent interest, in response to those who knew him as a geologist, the accidental occurrence of the now historic diamond at De Kalk, in the Hopetown Division of the Cape Colony, was brought to light in 1867. He was the first to determine its characters as a real diamond. Fully recognising the importance to the Colony of such a discovery, he suggested that it should be sent to the Paris Exhibition, and the Governor of the Colony secured it by purchase. The enormous wealth accruing to South Africa from this fortunate application of mineralogical knowledge and common sense is now too well known to be further dwelt upon.

In 1871, when he made his first visit to diamond fields, he noticed that Jagersfontein was being deserted by the diggers. He assured them it would be a good mine; and, when it was again worked eight years afterwards, it proved to be very productive. He also indicated the diamantiferous neck at "De Beer's New Rush," afterwards called Colesberg Kopje, and ultimately Kimberley. Subsequently, after he had become a Member of the Cape Parliament, he again visited the diamond mines—that of Kimberley in particular, with its deep shafts and numerous galleries; and he contributed many interesting notes to the local publications on the possible origin of the diamonds, and the causes of the disastrous "mud-rushes" deep down in the mine, as well as on some of the physical phenomena of the surface.

In the meantime he had visited the Gouph and the reported gold-bearing rocks of that and other localities, as far as Lydenberg, everywhere bringing his long experience and mature judgment to bear upon geological difficulties with more or less advantage. Dr. Atherstone took a warm interest in establishing the Geological Society of South Africa at Johannesburg in 1895; and he was elected an Hon. Vice-President at their first meeting.

We may well regard our old friend as one of those prominent geological worthies who have given their best energies to the elucidation of South African geology and to the benefit of their fellow-men. T. R. J.

## GEORGE JAMES ALLMAN.

Born 1812; Died 24th November 1898.

The last of the brilliant band of Professors who, under the Chancellorship of Lord Brougham, the Rectorship of Mr. Gladstone, and the Principalship of Sir David Brewster, a galaxy of talent unrivalled, made the scientific and medical sides of the great Scottish University everywhere known and esteemed, —the cultured and refined George James Allman passed away on the 24th November, at the age of 86. He was born in Cork, and spent his school days in Belfast; and his original intention was to study for the Irish Bar. But the natural bent of his mind was towards botany and zoology, and he graduated in Arts and Medicine in the University of Dublin in 1844. His earlier papers were partly botanical, and this led to his appointment (1844) as Regius Professor of Botany in the University in which he had studied. He also at this time gave much attention to the fresh-water Polyzoa, though there is considerable variety in his first fifty papers. Amongst the memoirs which his

fertile pen produced at the time, was that on the Anatomy and Physiology of *Cordylophora*, in which he gave an indication of his masterly treatment of the Tubularian Hydroids. When the somewhat sudden and lamented death of Edward Forbes took place, in the midst of his enthusiastic labours in his new sphere of action in the University of Edinburgh—surrounded as he was by congenial colleagues, and with a great future before him, Dr. Allman had sufficient influence in 1885 to obtain the appointment. Though, at first, opinions as to the successor of the brilliant Forbes were divided, yet Allman, by the publication of his volume on the Fresh-Water Polyzoa (Ray Society, 1856), a model of its kind in text and illustrations, at once gained support and disarmed criticism. His genial and courteous bearing, and the eloquence he displayed in his lecture-room, made his courses on Natural History exceedingly popular, and he, besides, occasionally conducted dredging excursions on Saturdays in the Forth with his students, a steam vessel being hired at Leith for this purpose. He was ever ready to explain and instruct as the dredge or otter-trawl came up, and many a young student of the "fifties" must retain pleasant memories of these delightful expeditions. Nor were his sympathies—with those who retired from active service as the trawl settled into action—more likely to be forgotten. Moreover, he devoted a short part of the next lecture to the most instructive forms met with in the excursion, illustrating his remarks by sketches on the blackboard. No professor, indeed, was more welcomed by the student of the day, and many still recall his once familiar figure as he hastened—not much before time—from Manor Place through the Grassmarket to the high class room in the Old University. Besides his University and more purely zoological work, he took much interest in the Scottish Fisheries, and was appointed a Commissioner to inquire into various subjects connected with the department. One of them was the investigation, along with Prof. Lyon Playfair, of the spawning-ground of the herring at the "Old Hake," near Crail. While pursuing this inquiry, he hatched the eggs of the herring quite easily in a simple apparatus. He also took an active part in transferring the zoological collections of the Old University Museum (so dear to many an old student) to their new quarters in the Museum of Science and Art. At the meetings of the Royal Society in Edinburgh he worthily upheld the traditions of the chair of Walker, Jamieson, and Forbes, his wide and accurate zoological knowledge and his fluent and graceful style combining to render him as trenchant as agreeable. Not a few regretted his somewhat early retirement from a post in which he was so fitted to distinguish himself, but considerations of health compelled him to resign his chair in 1870.

The comparative leisure which he enjoyed after his departure from Edinburgh enabled him to devote his whole energies to his favourite studies, while also advancing the general interests of science by his able Presidency of the Linnean Society, and by his occupation of the Presidential Chair of the British Association at Sheffield in 1879.

As a zoologist Prof. Allman brought to bear on his subject a cultured intellect, keen observation, philosophic spirit, and sound deduction, while his gifts in artistic delineation of the beautiful forms to which he specially devoted himself give his works a solidity and a charm all their own. These features, for instance, are prominent in his beautiful monograph of the Tubularian Hydroids for the Ray Society (1871-72), a work at once an honour to British zoology and an enduring monument to the talents and refined artistic touch of a master in the department. Marine zoology, indeed, has lost in him its foremost investigator and its most dignified expounder in our country. Himself a graduate in medicine, he fully appreciated, as shown in his introductory lecture in Edinburgh, the brotherhood of zoology and medicine, a feature the late Universities (Scotland) Commissioners have apparently misunderstood.

Of a somewhat delicate constitution, he shrank from those combats in which men like Prof. Huxley flourish, but in general society there were few so genial

and kindly, so full of sparkling Irish humour, and so refined and engaging in conversation. He was the last but one of the band of those older naturalists which included Edward Forbes, George Johnston, George Busk, W. Thompson, J. S. Bowerbank, J. Gwyn Jeffreys, P. Gosse, John and Harry Goodsir, Thos. Bell, Joshua Alder, Albany Hancock, Spence Bate, Thos. Hincks (still living), and others, who, by their genius and perseverance, have done so much to bring British marine zoology to its present position. Of all these none were closer friends than George Busk and James Allman, and they were worthy of each other.

W. C. M'Intosh.

## CARL WILHELM VON GUEMBEL.

Born February 11, 1823, at Dannenfels on Donnersberg in the Rheinpfalz; Died June 18, 1898, at Munich.

After half a century of active work in all branches of geological science, this eminent naturalist has passed away at the age of 75. After receiving a school education at the Gymnasium in Zweibrücken, he studied the art and science of mining at Munich and Heidelberg, and in 1848 received his first appointment at the collieries of St. Ingbert. His first scientific paper, however, was on his native Donnersberg. In 1851 he was called to the direction of the Land-Survey at Munich, and in 1879 was made "Oberbergdirektor" of Bavaria. To this kingdom his chief energies were devoted, and he received the reward of nobility in 1882. In addition to his civil posts, Von Guembel was honorary professor at Munich University and teacher at the Technical College.

The list of his scientific works is too large even for abstraction here. He is best known for his "Geognostische Beschreibung des Königreichs Bayern," which was begun in 1861, and developed into the memoirs of the geological survey of Bavaria. His "Geologie von Bayern," 1884-93, served as a geological text-book for the students of that country, as well as a summary most useful to foreign geologists. In addition to writing papers on mineralogical and petrological subjects, he invented instruments of precision for crystallographic optics, and Von Kobell perpetuated the memory of his labours in this field by the mineral name "Guembelite." The name *Guembelina*, applied to a fossil dactylopore, further recalls Guembel's extensive researches on fossils of obscure lower organisms, and his numerous papers on the Foraminifera.

The death was also announced early in December of William Colchester, formerly of Dovercourt, near Harwich, a well-known collector of Tertiary fossils. He it was who discovered the *Macacus eocaenus* described by Owen in 1839, and now in the Ipswich Museum. This was afterwards shown to be *Hyracotherium*. *Didelphis colchesteri* was another important find of Mr. Colchester's, and came with the *Macacus* from the Eocene sands of Kyson, Essex. Mr. Colchester became a Fellow of the Geological Society of London in 1857, and was of advanced age at the time of his death, which occurred at his residence, Burwell, Cambridge.

Dr. James Ingraham Peck, Assistant Professor of Biology in Williams College, and since 1896 Sub-Director of the Marine Biological Laboratory of Wood's Holl, Mass., U.S.A., died suddenly on November 4. He was born at Seneca Castle, Oneida Co., N.Y., August 10, 1863, and studied at Williams College and Johns Hopkins University. He worked chiefly on the food of marine fishes, also on the pteropods and heteropods collected by the "Albatross." His loss is deeply felt by his American colleagues.

The deaths have also been announced of:—On September 16, at St. Gallen, the Swiss Geographer, Prof. R. C. Amrheim, aged 53; the French Geographer, J. V. Barbier; on July 28, the Peruvian Geographer, Luis Carranza, M.D.; Dr. Hermann Endres, Privat-Docent in Anatomy at the University of Halle,

on July 30, aged 32; on December 11, Sir WILLIAM JENNER (*b.* 1815), who has been Her Majesty's physician for upwards of thirty years; JOHN W. KEELY, of Keely Motor notoriety, in Philadelphia, on Nov. 18; Dr. WILHELM KOCHS, Docent in Physiology at Bonn-am-Rhein, on Oct. 15; EDMOND MOLLERAT, Conchologist, at Saint Raphaël, France; on December 24, 1897, in Thorshavn (Faeroe Islands), the ornithologist, HANS C. MÜLLER, aged 79; ALEXANDER PILLIET, Curator of the Anatomical Museum of the University of Paris (Musée Dupuytren) and a writer on morbid anatomy, on Nov. 2, at Paris, aged 38; GEORGE VESTAL, Professor of Agriculture and Horticulture at the New Mexico Agricultural College, on Oct. 24, aged 41; Dr. DAVID A. WELLS, a writer on economics, and founder of *The Annual of Scientific Discovery* (1849-1886, Cambridge, Mass.), also author of a "First Principles of Geology."

# CORRESPONDENCE.

## VACCINATION.

Sir,—The letter of "U" in *Natural Science* for October gives one very striking exception to the rule laid down in *Natural Science* for September, that "no level-headed person with any capacity for weighing evidence doubts that vaccination, efficiently performed, affords an almost absolute protection against small-pox for a term of five to ten years according to the natural susceptibility of the individual." May I be allowed to pillory a few more?

Dr. William Gayton, giving evidence before the recent Vaccination Commission, in answer to Question 1755, expressed the opinion that "primary vaccination is a very fleeting protection indeed. As to the time that primary vaccination lasts I do not know, but I think it is a very short time."

Dr. R. A. Birdwood, whose experience of small-pox covers 12,000 cases, in answer to Question 31,191, stated that vaccination cannot be relied upon as an absolute protection up to any age whatever.

In addition to those who are not anti-vaccinationists, be it noted there are no less than four M.D.'s on the general Committee of the Anti-Vaccination League, one of whom is Dr. Charles Creighton, author of the article "Vaccination" in the "Encyclopaedia Britannica" (9th ed.), "The Theory and Practice of Vaccino-Syphilis," and other works.

Another is Dr. W. S. Tebb, whose recent work on "A Century of Vaccination" is full of facts marshalled in a masterly manner.

How many M.D.'s there may be as private members I have no means of knowing, but the recent addition of Dr. W. J. Collins, one of the Royal Commissioners and a signatory of the minority report, was recently published, and very naturally made much of.

Dr. Alfred Russel Wallace is another of those exceptions, every one of whom conscientiously objects in a greater or lesser degree to vaccination, and I for one hesitate to say that they do so "in strict proportion to" their "ignorance and inability to weigh evidence."

I quite agree that "were the money necessary to secure compulsory vaccination spent in a reasonable system of education of the masses as to the value of vaccination, it is possible that a larger percentage of vaccinations might be secured than under the present system," but it is very improbable.

One good would accrue from such a course, viz. the impossibility of gaining entrance into a scientific magazine of high standing, such as *Natural Science*, of a sentence like this—"The recognition of *the fact that vaccinia is merely attenuated small-pox*—proved again and again—only brings vaccination into line with what we know of other protective inoculations."—Yours truly, E. G. B.

[We have inserted the above from a sense of fair-play, but *Natural Science* is not the place for carrying on a discussion on the subject, and we must draw the line after alluding to two points:—(1) The duration of the protection afforded by primary vaccination is not to be settled by giving the opinion of

Dr. This or Mr. That, but by a study of attack-rates and death-rates at different age periods in definite epidemics amongst vaccinated and unvaccinated respectively (*see* Statistics of Sheffield Outbreak, by Barry, and of Gloucester Outbreak, by Bond); (2) as regards the identity of variola and vaccinia, *see* Klein's experiments recorded in Medical Officer's Report to the Local Government Board for 1892-93.—ED. *Nat. Sc.*]

## CLASSIFICATION OF BUTTERFLIES.

In *Novitates Zoologicae*, v. p. 374, ff. (1898) I published an account of the structure of the antennae of butterflies as a first instalment of my "Contributions to the Morphology of Lepidoptera." I am carrying out these researches on Lepidoptera in order to reach a basis of fact which will enable us to understand the various structures of the exoskeleton of Lepidoptera, and will thus also furnish a better basis for classification. As the number of species to be examined is so very great, oversights are likely to occur; but I hoped that, by inviting criticism of my paper, such oversights would be corrected by others, and thus our knowledge of the morphology and relationship of Lepidoptera be advanced. Criticisms in which errors as to facts are pointed out, or in which it is shown where and why my conclusions are fallacious, will have my earnest consideration.

Professor Grote, in the last issue of *Natural Science*, p. 440, does not give any reason why, in his opinion, the characters of the antennae are "valueless for taxonomy and phylogeny," and hence I have nothing to reply to this strange statement.

As regards Professor Grote's classification of Butterflies I merely say that it remains for Professor Grote to show that what he styles in Papilionidae vein ix. (absent from the other Butterflies, according to Grote) is *not* homologous of what he calls in the other Butterflies vein viii. (absent from Papilionidae, according to Grote). This fundamental question can be answered by a study of the development of the respective veins in the chrysalis.

KARL JORDAN.

ZOOLOGICAL MUSEUM, TRING, *December* 6, 1898.

In reply to Mr. Quail's criticism (*Natural Science*, p. 395, December 1898) of my conclusions as to the position of the Papilionides, a criticism based upon his reading of the characters of *Anosia*, I take up these one after another. The "structural blotch A" on the cubitus of *Anosia* may or may not be homologous with the cross vein of the Papilionidae. It is a slight extension at the extreme base, and is more prominent in *Heliconius*, where it really assumes the aspect of the remnant of a cross vein between cubitus and vein vi. In both cases it is situated much nearer the base of the wing than in the Papilionidae. Mr. Quail is correct that I had overlooked it (*l.c.* p. 394). But its exact nature and meaning are unimportant to my classification. I do not base my separation of the Papilionides upon the presence of a cross vein between cubitus and vein vii., but upon the existence of vein ix. in the latter group. There is no trace of vein ix. in *Anosia*, indeed there is no room for it. Again, the homology of the "structural blotch" of *Anosia*, of the Limnads, and Heliconians with the cross vein of the Papilionides would favour my theory that this structure is a part of a former general system of cross veins of the lepidopterous wing, as explained in the *Proc. Am. Phil. Society*. A cross vein would then have run in the Lepidoptera generally between v. and vii., intersecting vi., which latter vein has faded out; and thus the action by which the media has disappeared would have been repeated with variations in the region below the cubitus. The cross vein gradually fades out in the Papilionides themselves and is lost entirely in the higher forms, so I could not have placed any reliance upon its presence in the classification which I proposed. This, I think, disposes of objection "A." As to "B," the "rudimentary nervure," this is equivalent to my vein viii., and does

not touch ix.; with regard to "C," the position of vein i. in *Anosia* and the Limnads, I have especially alluded to this feature of the hind wings, and it again does not meet my point as to ix. of primaries. The main point I make, as against Scudder, Reuter, and others, is that they cannot logically interpolate *Papilio* with its vein ix. between the Blues and Skippers, as they attempt to do, because both these groups are without this vein ix., and, moreover, I have shown that the pattern of neuration of the two, thus violently separated, groups authorises the supposition that they are really related. The assumed dichotomy of the butterflies is another matter. It will be solved when we find vein ix., or when its traces are found in any other butterfly, of which neither Mr. Quail nor any one else has brought forward any evidence. I do not doubt that traces of vein ix. may be found in *Lagoa* or *Megalopyge* or *Cossus;* these are all Tineides (Grote), and I look for the origin of the Papilionides in this group. My theory of the movement of the veins in the wings of the butterflies and Lepidoptera generally is based upon the discoveries of Spuler and Comstock that the media is three-branched and the cubitus two-branched, while the radius is primarily five-branched. Some seeming discrepancies between Mr. Quail's interesting paper and my own communications may arise from a different point of view in this respect. But these are all side issues and do not affect the main issue, which cannot be decided in Mr. Quail's favour, I think, until he produces evidence that the other great group of butterflies, the Hesperiades, possess, or indeed ever possessed, vein ix. of the primary wings. It will not do merely to say that they have lost it; they may have sprung from an ancestry which had equally lost it, while the Papilionides must have sprung from an ancestry which had retained it.

A. RADCLIFFE GROTE.

ROEMER MUSEUM, HILDESHEIM, *December* 5, 1898.

[As our space for correspondence is very limited, this discussion cannot be prolonged here.—ED. *Nat. Sc.*]

## ARTIFICIAL FORMATION OF A RUDIMENTARY NERVOUS SYSTEM.

Thanking you for the honour done me by the publication of my paper in your review (November and December 1898), I venture to ask you to append the following note:—

Some vibrations of the neuroplasma might be explained by the osmotic currents of Bütschli, increased by the heat afforded by oxidations. Remak has observed that the axis-band has a reticular structure. In that case the theory of carbon dioxide should be rejected, but it would be profitable to make new experiments with an artificial protoplasm, obtained by a combination of the microscopic foam of Bütschli, which presents a *physical* analogy to the natural cytoplasm, and my own, which has a *chemical* analogy to the same, being formed by a mixture of the components of the plasmodium of *Fuligo septicum*, according to Reinke's analysis.

Perhaps the separation of the absorbed substances is effected by the partitions of the protoplasmic alveoli, and if that be true, the protoplasm itself might be compared to a gland, and even induced to grow (?) in a nutritive solution. In support of my position, I would refer to Dr. Loeb's address, the gist of which was published in *Nat. Sci.* xii. (1898) pp. 146-148. May I also correct two errata in my paper?

Page 336, instead of "negative variation of carbon dioxide" *read* "negative variation of nerve."

Instead of "Milne Edwards, *l.c.* vol. xiii. p. 5" *read* "Milne Edwards, *l.c.* vol. xiii. p. 58."

A. L. HERRERA.

MEXICO.

# NEWS.

The following appointments have recently been made: — Dr. Cleveland Abbe, jun., to be professor of geology in Western Maryland College; Drs. J. L. Ames, F. A. Woods, R. T. Atkinson, and F. R. Stubbs, to be assistants in the department of histology and embryology at Harvard Medical School, consequent on the resignations of Drs. H. P. Quincey and Elisha H. Gregory, jun.; R. T. Baker, assistant-curator of the Technological Museum, Sydney, N.S.W., to be curator of that institution; Dr. F. J. Becker of Prague to be professor of mineralogy at Vienna University; Dr. Boehmig as professor extraordinarius of zoology at Graz, Austria; Dr. A. Bühler to be privat-docent in anatomy at Würzburg; Dr. Capitan to succeed Gabriel de Mortillet as professor of prehistoric anthropology at the School of Anthropology, Paris; Dr. R. H. Chittenden to be director of the Sheffield Scientific School at Yale University, in succession to Prof. G. H. Brush resigned; Dr. Friedrich Dahl to be assistant in the Zoological Museum, Berlin; Dr. G. P. Eaton to be assistant in osteology in the Peabody Museum; Dr. H. Eggeling to be privat-docent and assistant in anatomy at Strassburg University; Dr. Marcus S. Farr to be curator of the zoological collection of the New York State Museum, Albany; F. G. Hopkins, late demonstrator in physiology at Guy's Hospital, to the new lectureship in chemical physiology at Cambridge University; Dr. Georg Karsten as professor extraordinarius of botany at Kiel University; Gregorius A. Kogevnikov as privat-docent in zoology at Moscow University; Dr. Fr. Kopsch as privat-docent in anatomy at Berlin University; Dr. Kriechbaumer to be curator of the zoological collections at Munich; Dr. W. Kulczycki as privat-docent in zoology at Lemberg University; Dr. Ernst Mehnert of Strassburg to succeed the late Prof. Endres as privat-docent in anatomy at Halle University; Dr. Heinrich Monke of Breslau to the Geological Survey at Berlin; Dr. Lubor Niederle to the newly-founded chair of Slavonic archaeology and ethnology at the Bohemian University, Prague; C. Sauvageau as professor of botany to the Faculty of Sciences, Dijon; Dr. Ernst Vanhoeffen to be assistant in the Zoological Institute at Kiel; Swale Vincent to be Sharpey physiological scholar and chief assistant in the physiological laboratory at University College, London; Dr. Franz Werner, well known for his work on "Coloration and Regeneration in Lizards," as privat-docent in zoology in Vienna University; Dr. A. Zalevski, as privat-docent in botany at Lemberg University.

Professor D'Arcy Thompson, C.B., of University College, Dundee, has been appointed scientific member of the Fishery Board for Scotland, vacant by the resignation of Sir John Murray.

Prof. Ray Lankester has resigned the Linacre Professorship of Comparative Anatomy in the University of Oxford. The past students of Prof. W. F. R. Weldon, of University College, London, are signing a testimonial to their former teacher in view of his candidature for the vacant chair. Among others whose names we have heard mentioned as candidates are Mr. F. E. Beddard, prosector to the Zoological Society of London; Mr. G. C. Bourne, who for many years

has been demonstrator and lecturer at Oxford; and Mr. W. Baldwin Spencer, formerly demonstrator to Prof. Moseley and now professor of zoology at Melbourne. The last mentioned is now visiting "the old country." The field, it will be seen, is a strong one.

Prof. Ch. Dépéret has been elected a member of the Paris Academy of Sciences, in the Section of Mineralogy, in succession to the late N. A. Pomel.

The Special Board for Biology and Geology of Cambridge University, has adjudged the Walsingham Medal for 1898 to J. Graham Kerr, B.A., Fellow of Christ's, for his essay entitled "Notes upon a Research into the Life-History of *Lepidosiren*." Proxime accessit: A. C. Hill, B.A., Trinity, for his essay entitled: "Enzymes and Assimilation."

Dr. O. Seydel, lecturer on Osteology at Amsterdam University, has resigned and returned to Germany.

Pharmaceutical Society of Great Britain. — Isaac Bayley Balfour, M.D., F.R.S., Regius Professor of Botany in the University of Edinburgh; Leonard Dobbin, Ph.D., Lecturer on Chemical Theory in the University of Edinburgh; Alexander Davidson, Montrose; James Laidlaw Ewing, J.P., Edinburgh; James Jack, F.L.S., Arbroath; George Lunan, Edinburgh; Thomas Maben, Hawick; and John Nesbit, Portobello, have been appointed members of the Board of Examiners for Scotland for 1899, under the provisions of the Pharmacy Acts, 1852 and 1868.

As a result of the litigation consequent on the Nobel bequest, a compromise has been effected by which the relatives of the deceased will receive about £211,000, while a sum of nearly £1,400,000 remains for the prizes. The interest on this will make five annual prizes of £8300 each. The subjects of the prizes are given in *Natural Science* for February 1897, vol. x. p. 139.

The subject of the first competition for the Nansen prize is some original research in embryology. The prize, which amounts to about £83, will be awarded at the annual meeting of the Christiania Academy of Science, on May 3, 1900.

The scientific staff proposed for the University of Birmingham, which is intended to absorb and expand the Mason College, includes professors of mathematics, physics, chemistry, metallurgy, zoology, botany, geology and physiography, mining, engineering, anatomy, physiology, bacteriology, and the usual medical subjects. In most cases there is to be a lectureship associated with each professorship. Mr. Chamberlain insisted at the preliminary meeting in November that a provincial University should be in some sense distinctive, "redolent of the soil and inspired by the associations in which it exists," and while recognising the prestige of the medical school and the necessity for giving science a prominent place, maintained that the special feature to be developed should be an organised commercial education. Another of his conclusions will also command sympathy—"that no University will be anything in which the teaching staff is insufficient or is starved."

A hearty response has been given to Lord Kitchener's appeal for £100,000 to found a college at Khartoum as a memorial to General Gordon. But considering the enthusiasm of the public and the amount of idle money in the country, it seems surprising that the money was not at once subscribed many times over. The nobler aspect of the temper of the hour was well expressed in Mr. Rawnsley's "Farewell to Lord Kitchener," in *The Outlook* of Dec. 10.

Under the bequest of the late A. W. G. Allen, the General Board of Studies of Cambridge University proposes to establish a research studentship of the value of £250, tenable for one year, and allotted alternately for scientific and for literary study.

At Cambridge University a John Lucas Walker Studentship in Pathology will shortly fall vacant. Candidates, who may be of either sex, should send in their names by January 19. The studentship is worth £200 per annum for three years.

On November 29th the University of Edinburgh conferred the degree of LL.D. on Lord Kitchener of Khartoum, and the University of Oxford conferred the degree of Master of Arts by diploma upon His Royal Highness Prince Christian Victor of Schleswig-Holstein, G.C.B., Magdalen College, Major in the King's Royal Rifle Corps.

The Maryland Geological Survey, the first Report of which was recently reviewed by us, has, during the past year, considerably enlarged its activities. It systematically collects statistical data concerning the mineral products of the State, which have an annual value of six to seven million dollars. The work of the Survey has been divided as follows :—Geology of the Piedmont Plateau, under the direction of Dr. E. B. Matthews ; Geology of the Appalachian Region, under Professor C. S. Prosser ; Geology of the Coastal Plain, under Dr. G. B. Shattuck ; Highways, under Dr. H. F. Reid ; Terrestrial Magnetism, under Dr. L. A. Bauer. Among the assistants are : A. N. Johnson, for highway engineering ; Cleveland Abbe, jun., for physiography ; B. Sollers and B. W. Barton, for botany. Topographic surveying is carried on in co-operation with the United States Geological Survey, one of whose officers, Mr. H. Gannett, has furnished an elaborate treatise on the aims and methods of topographic work for the report on the cartography of the State. Professor G. P. Merrill of the United States National Museum has reported on the building and decorative stones of Maryland. Extensive areal and economic work has been conducted in the western and central counties. The agricultural conditions, among others, have been considered, and the soils classified in co-operation with Professor Milton Whitney of the United States Department of Agriculture and with the Maryland Experiment Station. The connection of botany with geology is studied in co-operation with the newly organised State Horticultural Bureau.

The State geologist, Professor W. B. Clark, is also Director of the State Weather Service, and proposes to issue a series of reports, in co-operation with many of the United States Government officials. These promise to be of great practical value to the inhabitants of Maryland.

By the connection of the Geological Department of Johns Hopkins University with the Maryland Geological Survey, a great impetus has been given to the study of this science, and the laboratory has been much enlarged. Though Mr. G. K. Gilbert, who used to lecture on physiographic geology, has withdrawn, there are now at least five regular lecturers—namely, Professor W. B. Clark, Drs. Shattuck, H. F. Reid, E. B. Matthews, and Mr. Willis. Lectures have also been delivered during the academic year by Mr. H. M. Wilson, Mr. Fassig, Professor Hans Reusch, and Professor G. P. Merrill. Besides numerous short excursions around Baltimore, a permanent camp was established near Cumberland in the Appalachian Mountains, and, besides the regular instructors, special lecturers from the scientific bureaus in Washington were secured. It is noteworthy that attendance at the meetings of the Geological and Geographic Societies of Washington is considered an important part of the students' work. Members of this very active geological staff also give a course of lectures, with examination papers, to a hundred and ninety-seven teachers of Baltimore.

The multifarious activity, the broad-minded conception of the science, and the intelligent co-operation with all possible sources of help, that mark the geological institutions connected with Johns Hopkins University, reflect the greatest credit on the authorities of the University, especially Professor W. Bullock Clark, and may serve as a lesson to those Universities in this country that seem to have forgotten how essential growth and development are to a continued existence.

The Government of Natal has decided to undertake a complete Geological Survey of the Colony, mainly for the purpose of developing coal and other mineral resources of the country. The services of Mr. W. Anderson have been engaged as Government Geologist to superintend the Survey. Mr. Anderson has for some years been employed on the Indian Geological Survey, and previously served in a similar capacity in New South Wales.

The biological laboratory of the Brooklyn Institute of Arts and Sciences, at Coldspring Harbor, Long Island, has now been established for nine years, and has recently begun a systematic biological survey of the locality. The general outlines of the fauna and flora are sketched by Prof. C. B. Davenport in *Science* for November 18, 1898 (vol. viii. pp. 685-689).

Since her return from Cuba. the U.S. Fish Commission steamer "Fish Hawk" has been working under the command of Lieut.-Commander R. G. Davenport, U.S.N., and the scientific direction of Prof. H. C. Bumpus, in Narragansett Bay and around Block Island. Various problems presented by the marine fauna have been studied, especially those connected with the breeding and distribution of star-fish.

The Swedish Government has approached Her Majesty's Government in connection with a hydrographical conference, for the purpose of considering questions affecting the fishing interest. The Swedish Government is understood to be negotiating also with other powers.

Mr. Alfred L. Jones, says the *British Medical Journal*, has offered £350 a year to found and maintain in Liverpool a laboratory for the study of tropical diseases. This will be associated with the Royal Southern Hospital and University College, Liverpool.

Hyderabad is to have a Pasteur Institute adjoining its hospital and medical school. It is expected to be open for patients before June.

*Nature* learns from the Trinidad *Bulletin of Miscellaneous Information* that Dr. Morris, whose appointment as superintendent of the Botanical Department for the Lesser Antilles we recently announced, will have the control of the following stations : Barbadoes, Grenada, St. Vincent, St. Lucia, Dominica, Montserrat, Antigua, and St. Kitts. The Jamaica, Demerara, and Trinidad stations will at present remain independent, and a new station, under the control of Trinidad, is to be founded at Tobago.

At a meeting in Edinburgh on November 8th, a committee was appointed to consider the feasibility of establishing a Scottish Zoological Garden. The idea of a "Zoological Society" was mooted, but did not, we are pleased to learn, find support. There are already three or four societies in Edinburgh which have to do with Zoology, and any attempt to insinuate another would simply alienate the sympathies of those who would be glad to see a well-considered Zoological Garden instituted. A committee, including Prof. Cossar Ewart, Dr. Ramsay Traquair, Prof. A. E. Mettam, Mr. Fairgrieve, Mr. W. S. Bruce, Mr. Hope Findlay, and others, was appointed ; and we wish them success. We venture to predict that a successful site is to be found in the direction where holidayers do most resort. Proximity to the sea would also be a great advantage. We hope the enthusiasts and the capitalists may come to terms, and that more may soon be heard of this excellent scheme.

The Brighton Aquarium, which has long failed in promoting its original objects, is now in liquidation. We understand that negotiations are in progress for the purchase of the property by a Winter Garden syndicate. At the same time, some energetic citizens of Brighton are promoting a scheme for the establishment of a Zoological Garden on one of the hills behind the town.

In reference to our note last month (xiii. p. 368) on the Frank Buckland collection, we are pleased to learn that a memorial has been prepared for presentation to the Lord President of the Council and the President of the Board of Trade protesting against the removal and distribution of what was brought together after many years of laborious work.

With the view of rescuing the museum from oblivion, and raising it from its present abject position to one of usefulness, it is proposed that it should be made part of the duties of the inspectors of fisheries to preserve and deposit in the Museum of Economic Fish Culture any objects of permanent interest which may come under their notice, together with models of improvements in fish passes, fish-culture apparatus, etc., which may be useful for reference or record. It is also suggested that the secretary and the inspectors of the Fisheries Department, together with the representatives of the Fishmongers' Company, should be appointed visitors to advise on and aid in the efficient management and development of the museum.

The memorial has already been signed by the chairmen of the leading fishery boards throughout the country, and other associations and individuals interested in the fisheries, including the Duke of Richmond, Sir Edward Birkbeck, Sir W. Priestley, Sir Thomas Walpole, Sir George Macpherson-Grant, Lord-Justice A. L. Smith, the prime warden of the Fishmongers' Company, the president of the Fly-Fishers' Club, and representatives of all the leading fishing clubs.

Professor Trail, of Aberdeen University, has set on foot a movement for the formation of a Society and the institution of a Natural History Museum at Aberdeen. The following are amongst the objects of the Society :—(*a*) Collections from the district to illustrate with the utmost possible completeness the local natural history, and history of man from his earliest appearance within the district to the present time. (*b*) Series of types carefully selected to illustrate the leading features of natural history and of man's progress in their wider relationships elsewhere than in the locality; these series to be made of use for teaching. (*c*) Specimens and preparations mounted in boxes, constructed for easy and safe transport; these specimens, etc., to be selected as suitable for instruction in schools, and to be lent to teachers desirous to use them in teaching in their schools, the borrowers defraying carriage both ways, and making good any damage done to objects while in their custody.

Apropos of the Lithographic Exhibition at the South Kensington Museum it is interesting to note that for some time past experiments have been made which seem to show that similar results can be obtained by the use of prepared plates of aluminium. The importance of the discovery will be readily seen when one considers the relative weight and cost of Solenhofen stone and plates of aluminium. It has got to be shown whether the extreme delicacy of the work done of the process of Aloys Senefelder can be produced by the use of the metal.

Mr. John Morgan, of Hastings, who has for many years been engaged in collecting corals, has recently arranged his museum and made it accessible to the public in a suitable building connected with the Hastings Public Baths at White Rock. The hall, we understand, was originally intended for an aquarium, and Mr. Morgan uses some of the tanks for exhibiting various marine organisms. The collection is well labelled, and illustrated both by diagrams and Saville Kent's well-known photographs of the Great Barrier Reef. Mr. Morgan arranges for a weekly lantern lecture or demonstration, and has also secured the services of Mr. Connold, secretary of the Hastings Natural History Society, to conduct visitors round the museum.

In the museum of the Royal Agricultural and Commercial Society of British Guiana, at Demerara, various changes have recently been introduced. The exhibited series of birds has been revised according to the British Museum Catalogue, and over 200 specimens have been remounted by Gerrard and Son of

London. Other groups have been partially revised, so far as is possible in the absence of modern literature. It is hoped that the issue of a revised edition of the British Museum Catalogue of Fishes will enable the curator to work up those animals as completely as the birds; meanwhile a comprehensive collection of British Guiana fishes is being made, and preserved for the most part in formalin. Exhibition space in this museum has been extended by the addition of an upper gallery. Chief among recent acquisitions is a large series of rocks collected in the N.W. District by J. B. Harrison and H. I. Perkins, to illustrate a Government report. The chief difficulty in the curatorial work of this museum is presented by atmospheric changes and over much moisture. It is satisfactory to learn that many inquiries are made at the museum both personally and by correspondence, and that it is becoming more and more a general educating force in the colony.

Mr. Th. Masui reports to Baron van Eetvelde, Secretary of State for the Congo Free State, on the progress of the Congo Museum at Brussels. Over two thousand plants have been collected, of which about five hundred are new to science. Zoological and geological collections are also accumulating, and are being worked up already. Much is expected from the expeditions conducted by Major Cabra in the province of Mayomba, by Lieutenant Lemaire *en route* for Katanga, and by Commander Weyns to the Cataracts and Mid-Congo; and many other enterprises both scientific and practical are contemplated, or have actually begun. The practical side of the museum has two purposes in view—to present a complete collection of the products which the Congo furnishes, and to exhibit the materials which may be advantageously imported there.

Mr. J. G. Robertson has presented to the Edinburgh Museum of Science and Art the unique Skeleton of a Carboniferous Labyrinthodont, *Keraterpeton galvani*, from the Jarrow Colliery, Kilkenny, described and figured by Mr. A. S. Woodward in the *Geological Magazine* for 1897, p. 293.

The *Toynbee Record* for December 1898 gives some account of the excellent work which has been done by Miss Hall, Curator of the Whitechapel Museum, in rendering the collection available for the purposes of elementary education. Parties of school children, under the charge of their teachers, visit the museum, and have demonstration lessons. "Such an elementary museum," Sir William Flower said, "should be in every district in London. It should be the nucleus of the Natural History teaching in the schools, and a stepping-stone to the understanding of our larger museums."

A number of valuable donations have just been made to Dundee Museum by the Egypt Exploration Fund. These consist of an outer and inner mummy case, found in the necropolis of Ha-Khenensu, five tablets from Dendereh, and a large number of interesting archæological objects illustrating the religious and domestic life of ancient Egypt.

Miss Anna T. Jeanes has presented the Philadelphia Academy of Natural Sciences with $20,000, the income to be used for museum purposes.

Four large cases of manufactured Russian antiquities were recently despatched from Warsaw to this country for the benefit of English antiquaries. There is a regular trade in such objects at Kazan. *Verb. sap.*

The scientific societies of Plumstead and Woolwich have formed a committee to urge upon the Library Commissioners the formation of a museum or museums for the locality. The secretary is Mr. T. R. Marr, 6 Russell Place, Woolwich.

In our last number we gave a short account of the proceedings at the International Conference on Scientific Literature convened by the Royal Society. We did not think it necessary to say that we had abstracted this account from our highly valued contemporary *Nature*, since we assumed that

the *procès-verbaux* were public property, and that copies would be distributed to the press, especially the scientific press, in due course. No copy has yet reached us, and we gather from *Science*, as well as from other sources, that no attempt has been made by the Royal Society to furnish the scientific public with any account of the work carried on by this Congress. We now recall the strange fact that the elaborate "Report of the Committee of the Royal Society of London, with Schedules of Classification," though bearing date March 30, 1898, was never heard of by many of those most interested until late on in the year (*vide* articles in *Science*, and by Prof. Victor Carus in *Zoologischer Anzeiger*). It seems to us that the Royal Society does not realise its responsibilities. Why this shrinking from the public gaze? Are the members of the Committees so afraid of criticism? This is a scheme that appeals to the whole world of science; it will have to be supported by money; it will require the ardent co-operation of numerous individuals. To say the very least, it is not wise of the Royal Society to put on its usual airs of superiority and indifference in a matter of this kind. We have excellent reason for believing that the eminent and courteous Secretaries of the Royal Society are not responsible for this darkness where there should be light. Who, then, is the culprit?

In changing their treasurer the Royal Society of London has made an important alteration in its arrangements. From time immemorial the treasurer has been the deputy of the president in the latter's absence, but in the future the deputy of the president will be one of the vice-presidents, as is the custom in the majority of other societies.

The Report of the Council of the Palaeontographical Society adopted at the Annual General Meeting on 17th June, was issued on 21st November, so that the news contained in it is somewhat musty. It is pleasing, however, to learn that the accessions to the ranks of the Society have more than balanced the losses that had arisen from death and resignations; also that £40 : 14 : 0 has been obtained through sale of back stock. Within the next ten years the stock will, it is anticipated, be exhausted. Members can procure at a reduced rate publications more than ten years old; and separate parts, where a sufficient supply exists, can be bought for a sum dependent upon the number of plates. The new members of the council are W. Hill, J. Hopkinson, F. W. Rudler, and D. H. Scott. The president is still Dr. Henry Woodward, and the secretary Rev. T. Wiltshire, 25 Granville Park, Lewisham, London, S.E. The annual subscription is one guinea.

At the meeting of the Geologists' Association, London, on 2nd December, A. M. Davies read "Contributions to the Geology of the Thames Valley." (See p. 14.)

The opening conversazione of the Dublin Naturalists' Field Club was held on October 18; that of the Belfast Club on November 2. At each function Dr. R. F. Scharff and Mr. R. Welch exhibited specimens of *Mysis relicta*, a fresh-water shrimp recently dredged in Lough Neagh. Among the numerous other exhibits, testifying to the activity of both societies, we note *Elatine Hydropiper*, recently discovered in the Lagan Canal, and exhibited by J. H. Davies and W. Gray; new species of foraminifera from the Pleistocene clay of St. Erth, Cornwall, by Joseph Wright; land and fresh-water shells collected in Kerry, and living specimens of the Kerry slug, *Geomalcus maculosus*, by R. Welch.

At the second ordinary meeting of the Scottish Microscopical Society on December 16, a communication was read on "Changes occurring in some cells of the newt's stomach during and after activity," by Dr. E. Wace Carlier, B.Sc.

The officers of the Swedish Geological Society for 1899 are:—President, Prof. A. E. Törnebohm; Secretary, Dr. E. Svedmark; Treasurer, Dr. G. Holm; with G. de Geer and E. Erdmann as Members of Council. At the

meeting on 1st December P. J. Holmqvist read a paper on the Rapakivi Granite of Rödö, near Sundsvall. This district shows numerous variations and interminglings of acidic and basic eruptive rock of post-archaean age . G. Gellerstedt exhibited sections of clay-pits at Ekeby, near Upsala; these were drawn on a natural scale, and showed that the distortions in the clay could not be due to any slipping, but might be ascribed to the pressure of land-ice.

The Society for the Protection of Birds (3 Hanover Square, London, W.), has issued a leaflet, copies of which may be had from Mrs. F. E. Lemon, Hon. Sec., giving a summary of the proceedings at a conference held on Oct. 7, 1898. Mr. John Colan and Mr. W. B. Gerish dealt with imperfections in the Protection Acts; Mr. J. H. Allchin discussed the recent decrease of swallows and martins; Mr. Ernest Bell spoke of the value of birds of prey; and there were other papers.

In future the annual gathering of the learned societies of France is to take place alternately in Paris and the provinces. At Easter next it will be held at Toulouse.

The Economic Society of Mohrungen, near Königsberg, offers a prize of four thousand marks for the best work on the relations of electricity to living organisms.

We regret to see announced in *La feuille des jeunes naturalistes* the dissolution of the Societé des Naturalistes de Provence.

At a meeting of the Royal Physical Society, Edinburgh, on December 21st, the following communications were made:—On results of feeding *Drosera* with various chemical foods, by Miss L. H. Huie; on the age of the Shetland old red sandstone, by Dr. J. S. Flett; exhibition of and remarks on the eggs and embryos of *Ornithorhynchus*, *Echidna*, and *Ceratodus*, by Dr. Gregg Wilson; exhibition of eggs of *Xanthophilus bojeri* from Witu, British East Africa, by J. B. Dobbie.

At the meeting of the Geologists' Association, London, on January 6, Mr. H. W. Monckton will lecture on the glaciers and fjords of the Bergen district, Norway.

On December 15 Mr. J. G. Goodchild gave an address to the Edinburgh Geological Society on some recently exposed rock sections in Edinburgh, and gave approximate estimates of the thickness of the various series:—lower red sandstone, 1000 feet; Craigmillar sandstone, 500 feet; Balagon rocks, 350 feet; volcanic rocks of Arthur Seat, 750 feet; Abbeyhill series, 500 feet; the limestone series, 2000 feet.

On 6th December, Professor G. B. Howes lectured at the Free Public Museum, Whitechapel, on "The Story of a Thigh Bone." Forthcoming lectures are:—10th January, Dr. E. Starling, "How we digest our Dinner"; 7th February, F. A. Bather, "A Piece of Limestone"; 7th March, Miss Cora B. Sanders, "The ways in which Animals warn their Enemies and signal to their Friends"; 11th April, P. Chalmers Mitchell, "Brain." Lecturers at this Museum are sure of a good audience, and the curator, Miss Hall, is always glad to hear of anyone willing to help in this good work.

Several series of Lectures have been delivered during the past autumn at the Manchester Museum. Prof. F. E. Weiss had for his subject "Darwin's Botanical Work on Movement in Plants, Insectivorous Plants, and Fertilisation in Flowers." Prof. S. J. Hickson discussed the "Extinction of Species." Mr. Hoyle begins a Bank Holiday course on "Aquatic Mammals" on Boxing Day, including a special lecture for children, entitled "Water Babies," on January 7th. On January 21st, 28th, and February 4th, Prof. Boyd Dawkins is to lecture on the "Physical Geography of Britain in the Tertiary Period." He is also giving short addresses in the Museum on various Saturday and Sunday afternoons.

On December 3rd Mr. J. Arthur Thomson lectured to the Edinburgh Health Society on "Facts of Inheritance"; on December 11th Dr. Richard J. A. Berry lectured on "The Nervous System, its Uses and Abuses"; and the course closed on December 17th with a lecture by Dr. George R. Wilson on "Rational Enjoyment." The previous lectures in this (the fifteenth) course were on the following subjects:—"Crime," by Lieut.-Col. M'Hardy, Chairman, Prison Commissioners for Scotland; "Corpulence," by Dr. J. C. Dunlop; "Lord Lister and his Work," by Mr. Alexander Miles; "Milk as a Vehicle for the Spread of Disease," by Dr. James Foulis. The last-named has been the subject of a prolonged and animated discussion in the *Scotsman*.

On the 28th November, Mr. C. W. Andrews of the Natural History Department, British Museum, gave a lecture before the Royal Geographical Society on his recent exploration of Christmas Island, the lovely islet which lies in the Indian ocean about 190 miles south of Java. It rises from the summit of a submarine ridge separating two great abysses, the western extremity of the ridge forming the Keeling or Cocos Islands. The climate is delightful, the vegetation luxuriant, the fauna fairly rich, including some new birds and mammals. But it is in its rich deposits of phosphate of lime that the wealth of the island chiefly consists.

The following are among the lecture arrangements at the Royal Institution before Easter:—Sir Robert Ball, 6 lectures (adapted to young people) on Astronomy; Professor E. Ray Lankester, 10, on "The Morphology of the Mollusca"; Mr. A. Henry Savage Landor, 3, on "Tibet and the Tibetans"; Dr. Allan Macfadyen, 4, on "Toxins and Antitoxins"; The Rt. Hon. Lord Rayleigh, 7, on the "Mechanical Properties of Bodies." The Friday evening meetings will begin on January 20, when a discourse will be delivered by Professor Dewar on Liquid Hydrogen.

The year 1899 is the Centenary year of the Royal Institution, and arrangements are being made with a view to its celebration in a fitting manner.

Mr. P. W. Christian, who has spent much time in Samoa and other Pacific islands, and has made a study of Polynesian dialects, lectured at Eton College on October 29. That which most took the fancy of his hearers was his account of how he introduced cricket into several of the islands. The population takes such an interest in the matches that the partisans of the losing side try to loot the houses of the winners. Result, a free fight. Another feature of the game is that every able-bodied man has an innings. Hence a match lasts for many weeks, which, considering its usual termination, may be considered fortunate.

Sven Hedin is classifying his geological specimens, which he will present to the High School of Stockholm, and is preparing a detailed account of his journey from Kathgar to Khotan for *Petermann's Mittheilungen*. His archæological collection and manuscripts will be arranged by Professor Grunwedel, and exhibited in the Berlin Museum, whilst Dr. Ekholm is dealing with the meteorological notes. The maps and charts, covering 552 sheets, have been confided for enlargement and reproduction to Justus Perthes, of Gotha. Dr. Hedin proposes to start on his next journey of Asian exploration about the middle of 1899. He intends to cross the Taklamakan desert twice, thoroughly explore one of the largest rivers of Turkestan, and again study the interesting Lob Nor problem. The most important part of the work will, however, be explorations in the north and interior parts of Tibet. Dr. Hedin hopes to be able to spend a winter in some of the highest alpine regions of Tibet at a height of about 15,000 feet. Then he will pay a visit to the new Viceroy of India, and will return over Himalaya, Karakoram, and Kashgar. Dr. Hedin will again go alone, and he calculates that his three years' travel will cost no more than £2500.

Dr. Chas. F. Millspaugh, of the Field Columbian Museum and of Chicago University, is now undertaking a fourth expedition to Yucatan to study the flora of the interior.

The German Deep-Sea Expedition has already obtained some interesting results. In some of the deep-sea deposits, as well as in samples of water from the greater depths, many forms of bacteria have been observed. By the use of closing nets it has been shown that many crustaceans and fishes, supposed by earlier expeditions to live on the bottom, really belong to the intermediate waters. North of the Canaries the Josephine and Seine Banks rise steeply to within 100 fathoms of the surface. Soundings and temperatures were taken around the Seine Bank, and large numbers of hydroids, antipathids, and the crinoid *Antedon phalangium* were dredged. The last-mentioned was previously well known from the same locality, having been dredged there by the s.s. "Dacia" in 88 fathoms. Continuous observations have been made on temperature, on specific gravity of surface, and when possible of deeper, waters, as well as on density, colour, and transparency of the water, on ocean currents, and on atmospheric changes.

The Prince of Monaco's new yacht, the "Princesse Alice," which left Havre on June 23, made a successful voyage in high northern latitudes and returned to Havre on September 20. Dredgings at great depths were poor, but at lesser depths rich and varied.

Messrs. Chevalier, botanist, and Péjéal, geologist, have accompanied General de Trentinian on an expedition to explore French possessions in the Soudan.

*Science* informs us that Dr. W. J. M'Gee, of the Bureau of American Ethnology, and Prof. W. H. Holmes of the U.S. National Museum, have returned from explorations in the southern sierra region of California, where important collections were obtained for the museum and observations made on the surviving Indians of the district. Dr. J. Walter Fewkes of the same bureau is now carrying on researches among the Hopi Indians.

News has been received that Dr. H. O. Forbes, of the Liverpool Museum, and Mr. W. R. Ogilvie Grant, of the British Museum, safely landed on the island of Socotra on December 6.

Dr. Donaldson Smith, the American traveller, well known for his journey to Lake Rudolph, and more recent travels in Mongolia, proceeds to Somaliland to collect "big game" and birds for the Gaekwar of Baroda, who is desirous of adding to his already fine Museum of Natural History. He is accompanied in his trip by Mr. Carlile Fraser, of Paisley, who has had long experience of African travel in Nyassaland and Uganda.

In connection with Sir Clement Markham's appeal for subscriptions to a National Antarctic Expedition, there seems some hope of an important nest-egg. When Baron Oscar Dickson made offer of £5000 towards an Antarctic expedition, which was to be under the leadership of the elder Nordenskjold, Sir Thomas Elder offered a similar sum. The project fell through at the time, but it is said that before his death Sir Thomas deposited the above sum in an Adelaide bank, there to wait till the times were ripe.

Mr. F. H. Knowlton, of the U.S. National Museum, communicates the following note to *The Plant World* for November 1898:—"While collecting fossil plants in the State of Washington during the past season I discovered, about one mile north of the town of Liberty, a deposit nearly a foot in thickness made up almost entirely of gigantic palm-leaves. They are of the ordinary palmate or palm-leaf fan shape, with a petiole nearly an inch in diameter, and although no absolutely perfect specimen could be obtained, from the leaves being so matted together, there is evidence that the leaves must have been from four to six feet in diameter. It represents an undescribed species of *Sabal*."

The Council of the Marine Biological Association reports that the equipment of the laboratory and boats is now sufficiently complete to allow of a very much larger amount of scientific work being done, if the services of more naturalists could be retained for lengthened periods; but such development demands increased income. Twenty-one naturalists carried on research work at the laboratory during the year 1897-98. From the Director's report we gather that an Anglo-French Committee has been formed to investigate the physical and biological conditions of the English Channel during 1899, that £100 has been granted towards this by the British Association, and that Mr. H. N. Dickson will help in the physical part of the work. Dr. Allen gives a list of twenty naturalists who have worked at the laboratory; eight of these are not mentioned in the Council's report, owing, we suppose, to the difference of date. More attractions have been introduced in the aquarium, and an iron shed for storage has been added at the back of the laboratory.

On 10th December, Sir John Gorst addressed a large meeting of agriculturists at Cambridge, on the necessity of reform in the educational system of our agricultural districts. He referred to the evidence gathered by the Dublin Recess Committee as to what was being done in France, Belgium, Holland, Switzerland, and Denmark. The reports of the Commissioners showed that the chief reason of the agricultural prosperity of those countries, which so successfully competed with Great Britain, was the education of all classes, both adults and children, in the technical knowledge appertaining to their industry. He then sketched a system to effect the necessary reforms essential to the establishing of sound agricultural education in this country.

Attempts have been made more than once to utilise the thread of the spider in the same way as that of the silkworm, and about a century ago stockings were woven from it. The latest application is to balloon ropes for use by the military aeronauts of France, and a factory is in successful operation at Chalais-Meudon, near Paris. According to the *Board of Trade Journal*, the spiders are arranged by dozens above a reel, upon which the threads are wound. Each spider has to furnish 30 to 40 yards. The reddish and sticky outer cover is washed from the threads, which are then twisted by eights into yarn. This is both stronger and lighter than silk cords of the same thickness; but it is also much more expensive. The chief difficulty in these experiments has always been the feeding of the spiders.

The Government of New Zealand has issued an order protecting the eggs as well as the young of the interesting Tuatara lizard.

Caen Museum thought it was going to buy a fine meteorite weighing 750 kilos, and said to have fallen at Vierville (Manche) in April 1897. Alas! the meteorite never fell. It was the brilliant invention of a newspaper man.

The *Feuille des Jeunes Naturalistes* (35 Rue Pierre-Charron, Paris) proposes (from November 1, 1898) to lend the books of its scientific library to those subscribers who live in neighbouring countries.

A catalogue of these works is being issued, and up to this date twenty-four parts, comprising 33,000 numbers, have been published.

It is intended to add to the General Catalogue special catalogues on definite branches of Natural History, completing these by the acquisition of the various works issued on the subject. That on Collembola and Thysanura (anatomy, biology, systematic) was published on November 30. Those on the Tertiaries of Europe (1st part) and Cecidiology (study of galls from a botanical and an entomological standpoint) will be issued shortly. Other subjects are being worked out. The annual subscription to the Library, including the Magazine and the Library Catalogues, has been fixed at 16s. for English subscribers. Pamphlets can be borrowed for their postage (English stamps taken), and kept

for two months. The idea is novel, and should prove useful to those not connected with any large library. We hope that our colleague, Mr Adrien Dollfus, will meet with success.

The use of the globes, at one time a polite accomplishment, has of late been set aside in consequence of the great improvement in cartography and increased cheapness of maps. But the last year has witnessed a return to favour of the more natural method of representation, and interest in globes has been stimulated by the gigantesque proposals of Elisée Reclus. It is therefore an opportune announcement of Dietrich Reimer, Berlin, that he has now on sale a geologically coloured globe of the Earth, prepared by M. Pütz under the direction of Professor W. Dames. This shows not only the solid geology of the land, but also the deposits now forming in the seas, such as red clay and *Globigerina* ooze; this part of the work is based on the maps of Murray and Renard in the "Challenger" Reports. The diameter of the globe is 34 centimetres ($21\frac{1}{4}$ inches). The price is 25, 32, or 40 marks, according to the method of mounting.

The Government of British Guiana recently attempted to introduce the Indian buffalo (*Bos bubalus*) into that colony, but the experiment is likely to be a failure, since the animals cannot get enough water wherein to cool themselves at some seasons. Buffaloes from Indo-China have thrived and bred in Surinam for about four years, while in Cayenne they have done equally well for over ten years.

The friends of the late well-known Warwickshire geologist, the Rev. P. B. Brodie, have decided to place a window to his memory in Rowington Church, of which he was Vicar for forty-five years. The Hon. Secretary to the fund is Mr. J. Booth, Rowington Hall, Warwick.

In reference to a remark which we made (*Nat. Sci.* xiii. p. 435), Mr. George Abbott, Hon. Sec. of the S.E. Union of Scientific Societies, has informed us that the institution of a Postal Magazine Club has been attended by an *increase* in the number of periodicals bought by the members, and not by a decrease. This, from our point of view, is indeed a consummation devoutly to be wished.

The London School Board has taken a practical step to further the study of botany. Arrangements have now been made, which will come into operation in April next, whereby a gardener will forward to the schools botanical specimens required for teaching botany or for object lessons, or for the combination of drawing and object lessons.

The total output of gold ore in the United Kingdom in 1897 was, according to Dr. Le Neve Foster, 4517 tons, the total value at the mines being £6282. Copper mining is a decaying industry. The output of lead ore is also declining; last year it was only 35,338 tons, the smallest recorded during the last half century. The output of zinc ore, 19,278 tons, does not reach the average of the last quarter of a century. On the other hand, the output of coal last year was 202,129,931 tons, the highest hitherto recorded, while the output of iron ore reached $13\frac{3}{4}$ million tons.

The Entomologist of the United States Department of Agriculture has issued an appeal for separate authors' copies of papers on insects, so that these may be filed under their appropriate heads in the Library of his division. The publications of the division are very liberally distributed, but authors' copies are usually sent to individuals, and so become private property. Any that are sent in response to this very justifiable request should be marked "For the Library of the Division of Entomology." The valuable publications of this department, to which we so often refer, will prove a rich return to the senders.

# Natural Science

A Monthly Review of Scientific Progress

February 1899

## NOTES AND COMMENTS.

### Distribution and Morphology.

In an essay of 60 pages, entitled "Grundzüge der geographisch-morphologischen Methode der Pflanzensystematik," (G. Fischer, Jena,) Dr. Wettstein of Prague University has made a useful and suggestive contribution to the literature of systematic botany. The scientific systematist is no longer satisfied with adding new species or genera to the roll of names; he aims at establishing affinities between those already known, in a word, the study of their phylogeny. But there is great danger of losing oneself in theory and speculation, and any objective method of comparison is welcome. Palaeontology and ontogeny have proved of service in delimiting and determining the relationships of larger groups, as, for instance, the Fern allies (Pteridophyta), and the Gymnosperms and the Angiosperms. But in dealing with the higher subdivisions, genera, and species, these are useless, and it is here that Dr. Wettstein steps in with his geographic-morphological method. Morphology, though an excellent guide to affinities, is not always trustworthy, as, for instance, in the cases of seasonal dimorphism, which the author has described in certain Gentians, and species of *Euphrasia*, where late-flowering and early-flowering forms of the same species have quite differently shaped leaves, and show also differences in stem-development.

The object of the essay is to show how facts of geographical distribution may be used as a guide to the systematic arrangement of the members of a polymorphic group of species. As examples, Dr. Wettstein selects a group of European Gentians and Euphrasias. Thus the range of fourteen European species of *Gentiana*, section *Endotricha*, is depicted on one map, and the result is confusion. When, however, separate maps are devoted to each set of species, the distribution areas of which adjoin, it is seen that there are three distinct sets. As each set has, moreover, certain morphological characters in common, the author maintains that this combination of geographical and morphological considerations has elucidated three natural groups, and emphasises this conclusion by subordinating

the members of each group as sub-species of one species, which he regards as the archetype.

It is obvious that a satisfactory application of this method is possible only in cases where the geographical distribution has been approximately worked out, and, like any other method, its operation must not be pushed too far. It should at any rate prove of service to those who are working at floras of limited areas, and to such especially we would commend a careful perusal of Dr. Wettstein's essay.

## Irrelevant Generalities.

THE biological inquirer of to-day is like the fisherman of the old story who tapped a flask on the sea-shore and let loose a cloudy genie; he explores a concrete object, it may be the sperm-cells of a snail or the wings of an insect, and forthwith there emerges upon him and upon us a looming cloud of "allgemeine Betrachtungen," the genie of general biology, who will in nowise return to his flask. So far, doubtless, it is well that every research should have a wide horizon; but anyone who has read, say, the papers of the last five years relating to spermatogenesis, will surely agree with Mr. Bernhard Rawitz (*Arch. f. mikr. Anat.* 1898, liii. p. 20), that the generalising tendency is often as irrelevant as it is tedious. It is not necessary that everyone should point out every time how the often small item of new fact which has been securely established by his research bears upon the problems of cell-division, fertilisation, heredity, and the like, "ohne dass dabei ein erkleklicher wissenschaftlicher Gewinn abfiel." "Very many authors," says Rawitz, and he has our sympathetic concurrence, "seem to me like a geographer who describes the province of Brandenburg and hangs on to that a discussion of the whole physical geography of the northern hemisphere." We know of irrelevant detail, like that of the venerable geologist who could not discuss graphite without bringing in the life-history of the Faber family; but there are irrelevant generalities as well, and it is hard to say which are most exasperating.

## The Chinch Bug.

IN volume viii. of *Natural Science* (p. 376) reference was made to Mr. F. M. Webster's studies on the distribution of the Chinch Bug (*Blissus leucopterus*, Say) in North America. An exhaustive monograph on the insect by the same naturalist has just been issued as Bulletin 15 (N.S.) of the Entomological Division of the U.S. Department of Agriculture. Though intended primarily for the farmer's use—

the Chinch Bug being one of the six most destructive insects in the States—this work contains much to interest the general student of insect life. The severity of the attacks by the bugs on corn and grasses is largely due to their gregarious habits. They have but few natural enemies, but high mortality is at times caused among them by certain fungi, notably *Sporotrichium globuliferum*. Reference to the attempted utilisation of this fungus-disease for their destruction has already been made in *Natural Science* (vol. ix. pp. 6-7). Mr. Webster believes that artificial inoculation could only be worked successfully if the bugs were excessively numerous in wet weather or in damp situations.

With regard to the origin and diffusion of the species, Mr. Webster brings forward evidence to show that there have been three principal northward lines of migration from tropical America: a western, along the Pacific coast to California; a central, from the Gulf coast of Texas northward to Winnipeg; and an eastern, along the Atlantic seaboard to Nova Scotia. All the adults of the central migration have fully developed wings, but among the eastern race forms with vestigial wings are common. These latter have spread eastward and north-eastward, and in their progress through the New England States have acquired a habit of ravaging grass meadows, which is rarely practised by the bugs of the central migration.

Several species of *Blissus* inhabit Europe and Africa, but we can hardly accept Mr. Webster's suggestion that the ancestors of the American Chinch Bug were carried across the ocean by the equatorial currents.

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## Formation of a Human Race.

In his treatise on in-breeding and crossing in man ("Inzucht und Vermischung beim Menschen," Leipzig, 1897), Dr. Albert Reibmayr brings much erudition to bear upon an old problem, the respective rôles of in-breeding and crossing. Close in-breeding is necessary to fix and ennoble a race, but when it is prolonged, or if the material be poor, there is continual risk of weakness, infertility, and degeneration. But it is through in-breeding that a national character is evolved, and the problem of national eugenics is to watch for the crisis when crossing becomes necessary to obviate degeneracy. Thus in history there have been and must be alternating periods of dominant in-breeding and dominant cross-breeding. In America, the author indicates, a new race is being formed by complex cross-breeding, a new race which will lead mankind to new ideals in spite of the reactionary doctrine "America for the Americans." And behind this prophecy—which is always rather gratuitous—there is no small amount of solid anthropological measurement and statistic.

## Do Salmon feed in Fresh Water?

In the *Report of Investigations on the Life-History of Salmon* (Fishery Board for Scotland, 1898), edited by Dr. Noël Paton, there are two papers—by Dr. G. Lovell Gulland and by Dr. A. Lockhart Gillespie—which bear directly on the long-disputed question whether salmon feed while in fresh water. The Edinburgh workers agree with Miescher-Reusch (1880) in answering the question in the negative, thus differing from a well-known Scottish authority—Prof. W. C. M'Intosh. The evidence adduced in the Report is threefold:—(*a*) During the salmon's stay in fresh water the mucous membrane is in a state of desquamative catarrh which suggests a cessation of function. This is corroborated by the absence of zymogen granules in the pancreas, the fatty condition of the liver, the emptiness of the gall bladder, and the absence of all trace of food. (*b*) Experiment showed that the proteolytic and diastatic action of the digestive secretions was extremely low. (*c*) The number of bacteria in the gut is very great, especially during the warm months, and this is interpreted as probably due to the diminished acidity of the gastric fluids. This is, of course, merely a hint of the nature of the actual evidence, which seemed to us so convincing, when we first read it, that we were rash enough to think that the question was settled at last.

It seems that this is not so. In a paper in the *Zoologischer Anzeiger*, 1898, xxi. pp. 514, 515, 517-523, Dr. Alex. Brown, Lecturer in Zoology in the University of Aberdeen, who has had great facilities in studying salmon, maintains that though the river fish are not in a condition to feed voraciously as in the sea, they do feed occasionally, much depending on external conditions, such as the temperature, density, and volume of the water. He does not find any trace of the catarrhal conditions observed by Dr. Gulland, except as the result of post-mortem changes; but he finds microscopic foreign particles and crystals of carbonate of lime in the intestine, which are the indigestible remains of ingested food. Moreover, the presence of leucin crystals in abundance and of tyrosin in the intestine point to the decomposition of proteid by the pancreatic juice; and the pyloric and intestinal contents have a strongly active digestive character. Two more contradictory sets of results it would be hard to find; and we look with eagerness for a reply from the Edinburgh laboratory. It is difficult to believe that an expert histologist was deceived by post-mortem changes.

None of the investigators have as yet thrown light upon the fact that the salmon, alleged to be fasting, will still rise to the fly, or accept a prawn. We suspect that the explanation will be found to be psychological rather than physiological.

# The Mackerel and its Variations.

NUMBER 3 of volume v. of the new series of the *Journal of the Marine Biological Association* was published at Plymouth towards the end of November. It contains an important article on the "Variation, Races, and Migrations of the Mackerel (*Scomber scomber*)," by Walter Garstang.

This is an attempt to determine whether there are any racial peculiarities in groups of mackerel taken in different localities. Such peculiarities have not hitherto been recognised, even as between American and British representatives of the species; but it is clear that the establishment of such peculiarities would affect to a considerable degree our ideas concerning the migrations of this fish.

Mr. Garstang has determined the peculiarities of more than 1600 mackerel from various localities, in regard to ten chosen characters of which the following are the chief:—

A. The number of black transverse bars or stripes on one side (the left) of the fish. B. The number of transverse bars on one side (the left) which cross or meet the lateral line. C. The presence or absence of round black spots ("intermediate spots") between the bars of series A. The variation of this character is tabulated under two heads:—(1) The number of fish per centum which possess one or more of these intermediate spots, and (2) the total number of such spots per hundred fish (the left side only of each fish being considered). D. The number of rays in the first dorsal fin. E. The number of rays in the second dorsal fin, including any incipient finlets which are still partially connected with the fin by a low web or ridge, or which are merely closely approximated to the fin and erectile with it. F. The number of dorsal finlets, including all incipient finlets described under E.

The American mean values for several of the characters, especially A, B, C, and F, differ to such an extent from the British means, that there can be no further doubt as to the existence of racial peculiarities which distinguish American from British specimens of the mackerel. As compared with the British mackerel, the American fish possesses the following racial characteristics:—(1) A higher number of transverse bars, (2) much greater spottiness, (3) a smaller number of fin-rays in the second dorsal fin, and (4) a greater number of dorsal finlets. These characteristics are *average* distinctions, and do not suffice to distinguish every individual.

With regard to the British fish, the range of variation is very limited, especially in the case of the second dorsal fin, the greatest deviation from the general mean value of which does not amount to $\frac{1}{10}$ of a fin-ray in any sample of 100 fish. The total number of fish from the various localities is seen to be insufficient in this case to afford a basis for the establishment of racial differences. In the case

of the first dorsal fin the variation is greater, but here the difficulty of accurately determining in all instances the exact number of fin-rays present (owing to the extreme minuteness of the posterior rays) has also provided an obstacle to very definite results. Nevertheless Mr. Garstang comes to this "paramount conclusion":—That the mackerel which frequent British waters are not exactly alike in all localities, but possess certain average peculiarities which distinguish one local race from another. These peculiarities are greatest between the races of localities which are geographically remote, and least between those which occupy areas that are geographically contiguous. Between the mackerel of the North Sea and English Channel there are no differences at all; but the Irish race is distinctly divisible into two stocks, one of which is restricted to the west coast, the other to the south. A considerable amount of mixture takes place between the southern Irish stock and the fish which frequent the mouth of the English Channel. The western Irish stock represents more closely than any other race the primitive type of mackerel from which all, whether British or American, have been derived.

If these results be accepted or confirmed, the problems of the migrations of the mackerel and of its winter home are considerably simplified. The Irish fish in winter must remain off their own coasts, or they would lose their peculiarities by mixture with other races. The North Sea and Channel fish probably have the same winter haunts off the mouth of the English Channel—not too far to the westward, or they would mix with the Irish fish. That the North Sea fish migrate into the Channel in winter is rendered probable by the enormous concentration of mackerel in the southern part of the North Sea in autumn, and by the prolongation of the mackerel fishery far into the winter off the Devon and Cornish coasts of the Channel, long after the fish have disappeared from the North Sea and the Irish coasts alike.

The paper is one of the highest importance to both systematists and practical fishermen.

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## The Plymouth Laboratory.

OTHER papers contained in the same number of the *Journal of the Marine Biological Association* are:—"Report on Trawling in Bays on the South Coast of Devon," by E. W. L. Holt, with an Appendix by F. B. Stead. "Notes on *Pontobdella muricata*, the Skate-leech," by Hon. Henry Gibbs. "Notes on the Reproduction of Teleostean Fishes in the South-Western District," by E. W. L. Holt and L. W. Byrne. "The Great Silver Smelt, *Argentina silus*, Nilss. An addition to the List of British Fishes," by E. W. L. Holt, who gives the correct locality as 50° 20′ N. and 8° 25′ W., or about 75 miles true S. of the Old Head of Kinsale; depth 74 fathoms. This sensibly extends the range of

*A. silus* in a southerly direction; the specimen is now in the British Museum. Other occurrences recorded in this number are:—The Gobiid fish, *Callionymus maculatus*, near Plymouth; the Teleostean, *Phrynorhombus unimaculatus*, four miles S. of Plymouth Mewstone; the Gadid fish, *Motella cimbria*, from the stomach of a hake in the Bristol Channel; *Sepia elegans*, on trawling-grounds inside the Eddystone; *Mysis longicornis*, in Start Bay, S. Devon, not previously known outside the Mediterranean; *Mysidopsis angusta*, from the same locality, previously not farther S. than the Dogger Bank, but lately also recorded by A. O. Walker from Valencia Harbour, on the west coast of Ireland.

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## Colours of Cowries.

To *Knowledge* for December, Mr. R. Lydekker contributes an article upon the colours of cowries, illustrated by a full-page photographic plate. The interesting way in which the shells under consideration change during their growth from a thin-lipped form with a spire, as well shown by a series of skiagraphs in *Science Gossip* for June of last year, is briefly touched upon; while the alterations in colour, rendered so easy by the hiding of the older by the more recently formed whorls and the application of pearly matter to the outside of the latter, is discussed in some detail.

Mr. Lydekker is unable to throw any new light upon the object served by the colouring of adult cowries, nor does he suggest why the dark longitudinal bandings (which he calls transverse) of immature shells, presumably primitive and laid down by the edge of the mantle, should often be replaced by spots or uniform pigmentation derived from the pallial surface.

Nevertheless, the remarks show that many interesting problems remain to justify a further study of cowries, and the paper affords a useful illustration of the more striking types of coloration. Perhaps the most striking of the spotted series is not dwelt upon, to wit, the form with, for instance, a brown ground colour broken by white spots, the larger of which in turn have a very dark brown dot on their centre.

The plate would have been better entitled the markings of cowries seeing that it is not coloured, and it is hoped that it will not be taken as the best that photography can do for the conchologist. One is constrained to say conchologist, for the article ends with an uncalled for outburst against the biological method of studying molluscs. At the beginning of the change for the better about which Mr. Lydekker is not correctly informed, attention was naturally turned to the "so-called animal," our ignorance of the habits of which is so well brought out in the "colours of cowries," rather than to the points in connection with the shells which have been known for generations.

## The Fishes of Lake Tanganyika.

CONSIDERING the remarkable remnants of a Jurassic marine fauna discovered by Mr. J. E. S. Moore among the mollusca of Tanganyika, the collection of fishes made by him at the same time is of a very disappointing nature. According to Mr. Boulenger's detailed and beautifully illustrated report just issued (*Trans. Zool. Soc.* xv. pp. 1-30, pls. i-viii.), all these fishes are typical modern freshwater forms such as might be found in any part of tropical Africa. The most abundant and varied species belong to the well-known family universally described in standard works under the name of Chromidae, but here termed Cichlidae in accordance with the latest fad of literary "research." The Tanganyika types of this family, indeed, are so varied that they have necessitated the establishment of nearly as many new genera as were previously known from the whole of Africa. There are three species of the spiny eel-shaped fish, *Mastacembelus,* six siluroids, three characinoids, and one representative of each of the families Serranidae, Cyprinidae, and Cyprinodontidae. There is also an undetermined species of *Polypterus;* and Mr. Boulenger adds that *Protopterus annectens* has been recorded by Sir H. H. Johnston. In an appendix, however, Mr. Moore points out that his collection probably gives as inadequate an idea of the fish-fauna of Lake Tanganyika as would a haul on the rocky coast of the Isle of Wight in reference to the fish-fauna of the English Channel. Most of the interesting molluscs, sponges, and so forth, inhabit only the deeper waters of the lake; and the fishes lying at these depths are still entirely unknown. Moreover, large fishes which bite the paddles of canoes have been seen in the open water away from land, but none of these have yet been captured. Ichthyologists, therefore, look forward with great interest to the results of Mr. Moore's projected second expedition, when he will be equipped with more effective means of netting and dredging this and some of the other great lakes. Meanwhile, it is necessary to suspend judgment and postpone speculations as to the meaning of the remarkable fauna in question.

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## Zoological Notes from Japan.

VOLUME II. Part III. of *Annotationes Zoologicae Japonenses* was published on October 10, 1898. The first paper is by Seitaro Goto and Shinkichi Hatai, on Japanese earthworms of the genus *Perichaeta.* It describes sixteen new species; but to European workers the most interesting point will be the statement that all previous describers of *P. sieboldi,* from Horst to Rosa, have been mistaken as to the position of the spermathecae. A renewed examination of the specimens in European museums is called for.

Seitaro Goto, in the following paper, claims to prove that in the

star-fish *Asterina gibbosa* the various divisions of the adult body-cavity are derived from portions of the larval body-cavity, different to some extent from those portions that give rise to them in *Asterias pallida*. In so far as this is proved, we might infer that the development of the species in question cannot be utilised in constructing a morphology for the Asteroidea as a class. Professor Goto incidentally corrects certain observations and conclusions by E. W. MacBride. Unfortunately the paper still lacks complete illustration and proof from sections. This we are promised in a paper to be published in the *Journal of the College of Science, Tokyo*. In the third paper Professor Isao Ijima describes *Amœba miurai*, a new Rhizopod parasite of man.

## Nucleus and Cytoplasm.

In a paper read at the December meeting of the Royal Physical Society, Edinburgh, Miss L. H. Huie supplemented her previous observations (*Quart. Jour. Micr. Sci.* 1897, xxxix. pp. 387-425, 2 pls.) on the changes in the tentacles of the sun-dew produced by feeding. She finds that substances applied to the tentacles of *Drosera* affect either the cytoplasm alone, or both cytoplasm and nucleus. The cytoplasm reacts to physical irritation, and also to chemical substances which serve as foods. The nucleus is only affected by substances acting as food, the rate and amount of changes in the nuclein depending on the facility with which the food is absorbed. This agrees with Dr. Gustav Mann's view that the cytoplasm should be regarded as a specific environment secreted by the nucleus of each cell for the purpose of selecting and modifying potential food-stuffs which come into contact with it, and within physiological limits protecting the nucleus against detrimental conditions.

## The Natural History of the Congo.

Naturalists will learn with great interest that the Government of the Congo Free State has arranged to investigate the botany, zoology, anthropology, ethnology, and geology of the vast region of Africa under its jurisdiction, and proposes to issue a series of exhaustive monographs detailing the results. The series is to bear the general title, *Annales du Musée du Congo*, the nucleus of the Museum being the collection displayed at the Antwerp exhibition in 1894. E. de Wildeman and Th. Durand, of the Botanical Garden of Brussels, are occupied with the study of the flora. The zoologists who have already begun the task are G. A. Boulenger, Ph. Dautzenberg, Alph. Dubois, Seel-drayers, and Vincent. Anthropology is entrusted to Dr. Victor Jacques and Professor Stainier; while ethnology will be undertaken by Th. Masui, director of the whole work. The geology will be investi-

gated by Dr. Cornet of Mons and Professor Stainier. There will also be special monographs descriptive of different districts of the territory.

The first instalment of this great and important work has just appeared, and further issues are promised at intervals of about three months. The part now before us is the first fasciculus of Boulenger's description of the fishes of the Congo, a handsome quarto with nine plates, most of them folded and representing the animals of the natural size. The extraordinary fishes of the family Mormyridae are here treated with a wealth of illustration hitherto unattained, greatly extending our knowledge both of genera and species. No less than three genera and nineteen species are added to those already known. These fishes seem to seek their food either in mud or in crevices between rocks (unfortunately Mr. Boulenger gives us no information as to the circumstances of life of any of them); hence they exhibit most remarkable modifications of the curved and elongated rostrum which bears the small mouth at the tip. The preliminary labelling of these strange animals having been accomplished, biologists will now await with great interest some scientific observations on the phenomena they present for study. The other fasciculus published is botanical.

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## The Evolution of Immunity.

ACCORDING to Dr. Albert Reibmayr, it is one of the functions of the physician to exorcise the evil spirits of anxiety and pessimism, and this is the aim of his last pamphlet, "Die Immunisirung der Familien," Leipzig und Wien, 1899. It is "ein Wort zur Beruhigung," a protest against "ghosts." His argument is not a novel one, but it is powerfully stated. It is this. Morbid conditions and tendencies are inherited, but relative immunity is also inherited, and the struggle is between them. In the course of natural selection, keenest during the early years of life, the less immune tend to be eliminated, and the standard is thus raised. But in this struggle the most momentous factor is in the external conditions of function and environment, for if these favour the morbid inheritance the organism has to fight a battle with two fronts which is seldom hopeful. The families which lay themselves open to this double struggle tend to be eliminated, especially on the male side, for the females, who often live a more natural life, and are more enduring, may keep up the strain of immunity. It may be recalled that in Ehrlich's experiments on immunity to ricin poisoning in mice, it was only through the females that any hint of inherited (?) immunity was detected, the relative large size of the ovum, with its affectable non-idioplasmic material, having perhaps to do with this.

It may be said, then, that there are two main prophylactic processes—(*a*) the increase of constitutional immunity in the course of

natural selection, and (*b*) the living of a healthy life, along with which may be included the hygienic devices which make for the elimination of microbes. Our author believes that carefully discriminated statistics show a decrease in the number of severe and fatal attacks. The cry "Back to a more natural life" is, he says, an instinctive one, which is becoming more and more general, and it is the voice of reason. It is plain, however, that there is another side to the question, which Haycraft emphasised in his "Darwinism and Race Progress"—If the race eliminates its own eliminators (the disease germs) which have at least helped to make it what it is, if it becomes no longer susceptible to their eliminative action, what selective agents—even more discriminating, let us hope—are to take their place? But how the human race is to get along when bacteria have ceased from troubling is not a pressing problem, and Dr. Reibmayr does not raise it in his "Wort zur Beruhigung."

## Photographic Process Colour Printing.

THE publication of the well-known series of coloured photographic process views of scenery made it evident that similar processes would be soon employed for the illustration of zoological books, and we have just seen what we believe to be the first application of this method of illustration to birds. The pictures in question appear in "Birds that Hunt and are Hunted," by Neltje Blanchan, which was published in 1898 by Doubleday and M'Clure, of New York. Forty quarto plates of birds, with 360 pages of text, are issued at the low price of two dollars. The illustrations leave little to be desired, and some of them are perfectly lifelike, notably the Pintail Duck and the Teal, while the coloration of the Passenger Pigeon is perfect. These plates are produced by the Nature Study Publishing Company of Chicago, and are well worthy the attention of those who publish similar books in this country. All the exquisite pencilling of the feathers is shown in a way far superior to that produced by lithography, while the cost is infinitesimal as compared with that of an artist. We are sorry that the lithographic artist will have to go, but we are glad that birds can at last be presented with an accuracy that renders their identification easy.

## A Noble Gift.

THE Annual Report of the Curator of the Museum of Comparative Zoology at Harvard College has just come to hand. It is impossible to scan its pages without feelings of sadness and of admiration. For the last time in his official capacity as Director, Prof. Alexander Agassiz, the worthy son of an illustrious sire, renders account of his stewardship. This alone would make for sadness were it not lightened

by the knowledge that resignation was not intended to mean retirement from active research. On the contrary, as was announced some time ago, Prof. Agassiz has taken this step in order to devote his energies and talents, not to speak of means, to the pursuit of zoological investigation in distant and difficult fields. Zoologists must congratulate him on this resolution, deeply as they regret the severance of his official connection with the world-renowned museum which he has been so largely instrumental in creating. For all time the Museum of Comparative Zoology will be a noble monument to the name of Agassiz. But the parting act and the parting gift! These, indeed, command our admiration. As we read the deed of gift, its pathos and nobility seems to us indeed worthy of an Agassiz:—"I, Alexander Agassiz, of Cambridge, in consideration of one dollar and other valuable considerations to me, paid by the President and Fellows of Harvard College, the receipt whereof is hereby acknowledged, do hereby give, grant, and convey to the said President and Fellows the following described articles of personal property now belonging to me, and contained in or used in connection with the Museum of Comparative Zoology."

There then follows in the space of half a page a list of most of the contents of this museum and teaching institution.

The "valuable considerations" chiefly relate to permission being granted to Prof. Agassiz and his private assistants to use certain rooms of the museum for purposes of research, and as a storing-place for—later on to be made the permanent home of—his future collections. Many of us who have come in contact with Prof. Agassiz have experienced his liberality and self-sacrifice for the cause of science. What greater praise of this, his most recent act, can one give than this, that it is worthy of the man?

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## Mosquitoes.

THE alleged connection between Mosquitoes and Malaria has led to a special interest in the collection of various forms of Culicidae, and in this matter the authorities of the British Museum have printed a seven page tract of instructions how to collect. The chief points in the collection of Culicidae to be borne in mind are, not to use spirit, to pin specimens directly they have been killed, and to send specimens home as soon as possible, because they are liable to go mouldy if kept after death in tropical countries. One of the chief objects of this investigation is the determination of the various kinds of Mosquitoes, since the species belonging to the family are at present but little known. The tract, which applies to all species of Diptera, can be obtained by application to the Director of the British Museum (Natural History), London, S.W.

# The Hull Club Commences Author.

The Hull Scientific and Field Naturalists' Club is one of those local societies to whose energy, directed as it is into the most appropriate channels, we have often had pleasure in alluding. Therefore, although we are accustomed to deprecate the multiplication of minor publications in natural history, we are half inclined to welcome the appearance of the first of a series of annual *Transactions* which the committee has decided to issue. Indeed, our welcome becomes almost whole-hearted when we read that the aim is "to publish original papers and notes of *local* interest." For in these matters it is just those accurately-made local observations that have an abiding value for naturalists at large, whereas the general, even cosmical, lucubrations that fill up some publications of the kind are of interest chiefly, if not solely, to the writers themselves.

The contents of the present number coincide to such an extent with the aspirations of the founders that our welcome becomes actually a warm one. We entertain no doubt that these *Transactions*, if they continue as they have begun, will materially advance the scientific study of natural history in Hull and its neighbourhood. The first paper is by Thomas Bunker, on "The Natural History of Goole Moor and the Immediate Vicinity"; this is historico-pastoral, botanico-ornithological, zoologico-ecouomical, and shows that the marshland of Goole and Thorne Moor is a happy hunting-ground for many kinds of naturalists. H. M. Foster gives a useful and interesting account of "The Fishes of the River Hull," which are of twenty-one species. An exceptionally fine pair of antlers of the red deer (*Cervus claphus*), from the peat at Hornsea, is illustrated and described by T. Sheppard.

Extracts from the Secretary's Report are published, and from these we learn that the Club now numbers 127 members. Several of them are country members, residing more than ten miles from Hull, and admitted at a reduced fee. A good scheme recently carried out by the Club consists in practical demonstrations on such subjects as taxidermy, microscope preparations, preservation of larvae, and local fossils. J. F. Robinson, the recorder for botany, has compiled a Flora of the East Riding; while the entomologists have in preparation a list of the Macrolepidoptera collected within eight miles of Hull. These will be published in future numbers of the *Transactions*.

The present number is, correctly considered, a quarto, but measures $22 \times 14$ centimetres ($8\frac{1}{2} \times 5\frac{1}{2}$ inches). It contains viii. + 28 pages, and one plate. The date of publication, December 14, 1898, is printed on the wrapper; if it were also on the last page it would be still better. The price to non-members is 1s. We congratulate the anonymous editor on the general appearance of the number; at the same time, we would suggest that each annual issue should form an independent part, with fresh pagination.

## Stone Implements from Swaziland.

A FINE series of stone implements has recently been sent to England by Mr. Sidney Ryan. They were found near Darkton, in the tin-bearing gravels of the M'Babaan, or Embabaan, River, West Swaziland. The thirteen consists of siliceous schist, black fine-grained quartzite or chert, and of quartzites composed of grit and breccia of quartz, lydite, and jasper. There is also one of crystalline quartz. The large forms vary in length from $4\frac{3}{4} \times 3\frac{1}{4}$ to $8 \times 4\frac{1}{2}$ in., and weigh from $13\frac{3}{4}$ oz. up to 2 lb. $7\frac{1}{2}$ oz. Their shape corresponds to weapons found in England and France, being of a long-ovate, sharper at one end than at the other. Professor Rupert Jones has described and figured these interesting implements in the *Journal of the Anthropological Institute*, New Series, vol. i. It is not possible at present to assign any age to these implements, as the age of the gravel in which they were found has not yet been determined by geologists.

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## Danish Boulders.

THE investigation of the age of the boulders, as determining the existence of rocks intermediate in age between the Senionian of Jutland and the Lower Palaeozoic of Norway and Sweden, was first begun by Forchhammer about 1820. Of recent years Miss Ethel G. Skeat, of Cambridge, and Mr. Victor Madsen, of Copenhagen, have undertaken the investigation, and the results of their labours are now before us under the title of "On Jurassic, Neocomian, and Gault Boulders found in Denmark." The paper, which forms Part 8 of the second volume of *Danmarks Geologiske Undersogelse*, consists of 214 pages, 8 plates, and a map. Forty-three boulders have been examined, and their fossil contents carefully described by the authors. They have been determined as follows:—11 from the Lias, 1 from the Callovian, 24 from the Kimeridge-Portland series, 2 from the Neocomian, and 4 from the Gault. The general conclusion supports Forchhammer's theory, and the boulders are considered evidence of former deposits to the north of Jutland and in the Skager Rack region.

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## Coccospheres and Rhabdospheres.

THE nature of these bodies has been referred to in *Natural Science* from time to time, and we now have to record the appearance of the results of researches by Messrs. G. R. Murray and V. H. Blackman during a voyage to the West Indies in 1897. The authors agree with Haeckel

in regarding these bodies as Algae, but consider that all Coccospheres belong to one genus only, the same applying equally to the Rhabdospheres. Both are now described as "free unicellular algae, provided with an outer covering of calcareous plates, free from, overlapping, or readily separable from each other, the plates characterised by symmetrical excrescences or markings." Both are referred to the family Coccosphaeraceae, the Coccospheres being divided into two species—*C. pelagica*, Wallich, and *C. leptopora*, n.sp.; while the Rhabdospheres form *R. tubifera*, n.sp., and *R. claviger*, n.sp. Some beautiful figures by Mr. Highley accompany the paper, which is published in the *Philosophical Transactions of the Royal Society*, volume 190B.

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## No Freaks.

It is recorded in the daily press, whether accurately or otherwise concerns us little, that the divergent members of Barnum and Bailey's recently held a meeting, and drew up a memorial protesting against being called freaks. There are discontinuous variations among them, and some strange modifications, but no freaks. It is interesting to find that Dr. Hans Gadow of Cambridge has also been saying "no freaks." The original slight change in pigmentation which started the greenness of the tree-frog is not a spontaneous freak, as the Neo-Darwinians say; it was caused, Dr. Gadow assures us, by the direct influence of the sunlight. But no experimental evidence of this is given.

It seems late in the day to point out, as the author does, that there is nothing spontaneous in the sense of being causeless; or to emphasise against the Neo-Darwinians the universally recognised truism that environmental stimuli must be antecedents of all variations. But as the Neo-Darwinians include in their ranks not a few believers in definite variation — see even Weismann's "Germinal Selection,"—we rather resent being told that we must choose between believing in the inheritance of modifications, which we should gladly do if we could find evidence, and believing that the raw material of evolution may be summed up in the word freaks.

Dr. Gadow maintains that variations are as such adaptive, that variation and adaptation are fundamentally the same—an interesting antithesis to the equally extreme position of Virchow, that all variations are pathological. The author reproaches the Neo-Darwinians for simply taking variations for granted, as if there had not been many attempts (in Cambridge and elsewhere) to find out something about their occurrence, their nature, and their possible origin. And the strange thing is, that after making this reproach, he himself goes one better in postulation, for he begs the question, as it seems to us, by assuming adaptation as a cause, and not a result.

## More Misunderstandings of Weismann's Position.

IT seems as if we should do well to stereotype this heading. For the name of these misunderstandings is indeed legion, though Weismann's position is as clear as sunlight. In Haeckel's little book, entitled "The Lost Link," Dr. Hans Gadow returns to the charge against those who find themselves unable to discover any evidence of modification-inheritance. In his notes, which form almost half of the book, and no small part of its interest, the erudite morphologist of Cambridge champions Lamarckism. "The inheritance of acquired characters becomes very obvious," we read, "in the following example"; and being most open to conviction we hurry on breathlessly, to find that the offspring of *Protomyxa aurantiaca* (one of the simplest of the Myxomycetes, or slime-organisms) is as orange as its parent. Does Dr. Gadow really believe that this "example" has any bearing whatever on the problem at issue? Why not also cite the case of the orange-tree? There is no evidence that the orange colour is an acquired character, and it is only with great difficulty that the conception can be applied to any unicellular organisms, where the distinction between soma and germ-plasm is only incipient.

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## Variation in Sea-Anemones.

IN continuation of our note in last number (p. 12) on variation in a sea-anemone (*Metridium marginatum*), it is of interest to record Mr. H. B. Torrey's observations (*Proc. California Ac. Sci.*, 1898, i. pp. 345-360, 1 pl.) on *M. fimbriatum*, which is "practically identical" with the first-named species. Among 1971 specimens collected at random 48 showed asexual reproduction (monogenesis), some being at the same time sexually mature. Three modes occur—longitudinal fission, basal fragmentation, and budding from oesophageal and foot regions. Parker's suggestion that the monoglyphic and diglyphic types of *Metridium* may be of the value of varieties, "the products one of the sexual, the other of the asexual mode of reproduction," is not corroborated, for both types reproduce by fission, and both may result from fission. Indeed, of two buds which arose independently from a single basal fragment, one was monoglyphic, the other diglyphic. The variants are not varieties.

# ORIGINAL COMMUNICATIONS.

## On the Study of Plant Associations.

By Robert Smith, B.Sc.

It has long been recognised that a close connection exists between the kind of vegetation of a country and the prevailing conditions of climate and soil, and that changes in the one, either in space or in time (phenology), bring about corresponding changes in the other. Thus many works on the flora of a region have been supplemented by a general account of the conspicuous vegetation in the landscape, the chief trees, the nature of the stations, etc., correlating this with the general climatic conditions of the region. This general account, at first vague and unmethodical, has gradually become more and more organised into a definite survey of the "Vegetation," as contrasted with the "Flora." For long it remained hardly more than a branch of descriptive geography, employed as indicative of the climate and economic wealth of a country, but within recent years the subject has assumed an important biological position, and now may be taken to represent a description of the relations which exist between the plant-covering of a region and the conditions of life.

The floristic method of study, on the one hand, is more directly concerned with the historical development of the "flora." It attempts to answer the questions, How have these species originated? or whence have they come? It studies phylogeny and migration; and the botanical characters to which it directs attention are those on which we chiefly rely for indications of racial affinity, namely, those of the floral organs.

In considering the "vegetation," on the other hand, the essential characters of the species are those indicating adaptation to the environment, and are to be found mainly in the vegetative organs. Hence groups of similar adaptational form, "Lebensform" of German authors, need by no means coincide with natural families or groups of species. For example, *Empetrum* and *Erica*, or *Aloe* and *Agave*, possess similar "life-forms," and could be grouped as Ericoid-forms and Aloe-forms

respectively, yet their floral characters indicate widely separate genetic affinities.

Warming (1896) has recently subdivided the study of Plant Geography into two main branches based upon these two different points of view, flora and vegetation :—

(1) Floristic Plant Geography, which considers questions relating to origin, to past and present lines of migration—in short, to the distribution, past and present, of the species ;

(2) Oecological Plant Geography, which considers the life-forms of species, their association, and their relations to the life-conditions (heat, light, moisture, food, etc.).

The vegetation of any region is to be considered then as an association of plants bound together by the fact that they are all adapted to life in this region. The region may not be uniform throughout its whole area, but may include a great number of sub-associations, each characterised by certain forms of vegetation and determined by particular conditions of life.

Within each association there are various grades of successful adaptation, and accordingly we find that the plants group themselves as dominant, secondary, and isolated species.

The study of the vegetation has thus become a study of plant associations—the life-forms which constitute them, the conditions which determine them, and the relations between them.

The methods for this study are at present by no means well-defined, and probably from the nature of the subject will always remain more or less arbitrary. The differences in the methods employed by the various workers are largely due to the different standpoints from which they view the subject ; whether they regard the vegetation as a term in the description of the scenery, as an index to the meteorological and soil conditions of the country, or as an association of living organisms. This last aspect is the true biological one, and has steadily gained ground within recent years.

The first attempt to organise the study of " vegetation " was made by Humboldt in 1806. Before his time travellers had employed the dominant features of vegetation in describing the landscapes they had viewed. Humboldt tried to make these descriptions systematic. From the experience he gained in his wide travels he enumerated the following plant-forms as dominant in different landscapes :—palm, banana, cactus, orchid, mallow, bamboo, mimosa, aloe, grass, fern, lily, willow, myrtle, melastoma, and laurel. He further noted the variations in the proportions of the individuals of different species, how some were social and some isolated (1807, p. 15) ; and in his definition of the scope of plant geography he says, " C'est cette science qui considère les végétaux sous les rapports de leur association locale dans les différens

climats" (1807, p. 14). In his classical essay (1807), where he treats of the vegetation of Chimborazo, he shows how the differences in the plant-life at different elevations are dependent upon corresponding differences in temperature, humidity, barometric pressure, etc.

A. Pyr. de Candolle (1820) recognised the importance of noting all these facts of association. In his scheme for a flora of the valley of the Lake of Geneva he indicates the following amongst the facts which should be ascertained:—

"5°, La station, c'est-à-dire, la nature spéciale des localités relativement à la nature du sol, à son inclinaison, à son arrosement; la nature et la température des eaux; l'éclairement ou l'obscurité, etc.

6°, Ses variétés locales, c'est-à-dire, les changemens de formes observées dans les limites adoptées et leurs rapports s'il en est avec les localités.

8°, L'indication du degré de sa rareté ou de son abondance, le rôle qu'elle joue comme support, appui, ou ennemi des autres végétaux, la désignation de la plante comme éparse ou sociale; enfin, l'indication des espèces avec lesquelles elle a coutume à vivre."

This attempt to include all the observations on the distribution of the species in a flora has, so far as we know, not been carried out, since it was soon recognised, that many of the facts could from their nature be more conveniently recorded and studied apart from the "flora."

Grisebach (1838) more fully organised the methods for describing the physiognomy of the landscape, and for this purpose introduced the new term "pflanzengeographische Formation." A plant "formation," according to him, is a group of plants, such as a wood or a meadow, which forms a distinct and complete feature of the landscape. It may be characterised by one social species (a pine wood), by a group of similar social species (a pasture), or by an aggregate of species of manifold organisation, but having one general and conspicuous characteristic (an alpine meadow where all the plants are perennial herbs).

Thurmann (1849, p. 22) distinguishes between the flora and the vegetation of a country; after discussing the scope of each method of study he sums up as follows: "La Flore s'entend surtout du nombre des formes végétales distinctes qu'on y observe, la Végetation de leurs proportions et de leur association." He and other contemporaneous students of the relations between the plants and the subjacent soil (such as Unger, von Mohl, Sendtner, etc.) were well aware of the fact that slight differences in the soil conditions give corresponding differences in the frequency of the species growing there, and they employed a series of terms from social to isolated to indicate this.

Alph. de Candolle (1855) attempted to answer the question why some species are social and others not. This, according to him, is due to two causes:—

(1) The constitution of each species, some being able to crush out their neighbours in virtue of certain characteristics such as rapid growth, long duration, quick germination, shade, etc.

(2) The particular conditions of each station. He remarks, too, how many species have the social habit, even to the limits of their area of distribution, where they disappear suddenly and new forms take their place. He reasons that purely local causes seem to be of much more importance than general climatic conditions in determining the abundance of the individuals of a species at any particular point. It is owing to this fact that the study of social species is valuable in topographical botany. He also calls attention, as Humboldt had done before, to the difference between such a case as that of a northern pine forest, where one species is dominant, even to the exclusion of others, and such a case as a tropical forest, where a certain normal character is given by a mixture of many associated species.

Thus the scope of the subject had been foreseen and discussed by the middle of the century, but as yet its interest was almost purely geographical, and the biological element was hardly touched upon.

With Darwin a new period in natural science began. He directed attention upon organisms in nature, how they are adapted to their life, and how they struggle for existence. Since 1859 a steady stream of works have appeared treating of the anatomical and physiological characters of plants in relation to their habitat. In the bibliography at the end of this paper a few only of the chief of these have been mentioned as representative oecological works, dealing with such marked forms of vegetation as strand plants, aquatic plants, halophytes, desert plants, etc. (see List B). Such close associations as exist between symbions, and between parasites, epiphytes, and climbing plants and their hosts, have already an extensive literature devoted to them. The associations of plants and animals, especially plants and insects, have occupied the time of many investigators, even to the neglect of the more general study of plant association.

The first to apply the new ideas to the description of the vegetation of a particular region was Kerner (1863) in his work on the plant-life of the basin of the Danube. After describing the general characters of the region, he represents the vegetation as being made up of a number of "formations" (as defined by Grisebach), each "formation" characterised by some definite species (one or more), possessing adaptations which enable it to suit the particular conditions of environment and to dominate in the landscape. Thus the first steps of the method were:—

(1) To describe the general characters of vegetation.
(2) To enumerate the leading plant "formations" of the region and the plants which dominate in each.

(3) To describe the life-conditions of each "formation," and the forms of vegetation characteristic of it,—whether trees (with evergreen, needle-shaped, or deciduous leaves), shrubs, grasses, herbs (perennial or annual), thallophytes, etc.

Grisebach (1872) brought out his great work "Die Vegetation der Erde," in which he employs the same methods for the description of the plant-life of the whole globe. For this purpose he drew up a scheme of 54 plant-forms to indicate the characters of vegetation of each "formation." His classification was still on the whole geographical, for his "formations" were defined as subdivisions of the landscape ("pflanzengeographische Gliederungen der Landschaft"), and his "forms" as the adaptational forms of plant-life most conspicuous in the landscape.

Reiter (1885) modified Grisebach's scheme of plant-forms to reconcile it with later research in plant oecology. But all such classifications must remain more or less artificial, until we understand more about the nature of these "adaptations." Experimental and histological work on the subject, like that of Vesque, etc. (see List B), must be the basis upon which a true classification can be raised.

Drude (1888) introduced the terms now usually employed to indicate the relative frequency of species in a plant "formation." The term "social" is applied to the dominant species, whose individuals are so numerous that they seem to cover the whole of the ground, *e.g.* the heather on a moor. "Gregarious" plants are those which live in groups or patches in a "formation," *e.g. Anemone nemorosa* in a wood. The other terms "copiously intermixed," "sparsely intermixed," and "solitary" explain themselves. Pound and Clements (1898) point out how often the eye may be deceived with regard to the relative value of the secondary species of a "formation," and they advocate the actual counting of the individual plants within type plots. Blomqvist (1879) for a similar purpose has attempted a graphic method of representing the secondary species in a formation, by mapping typical small areas on a large scale.

Thus methods were becoming well-organised, and several important applications of them had been attempted,—in this connection "Das Pflanzenleben der Schweiz" (1879), by Dr. Christ should especially be noted.

But the term "formation" was found to be insufficiently defined, and it came to have a different meaning with different authors. The "formations" described as units by Grisebach (1872 and 1875) were split up into finer and finer divisions according to the minuteness of the study given to them. Thus Hult (1881) has described almost fifty for Finland alone. To distinguish them he employed a system of nomenclature which is now frequently adopted, where each "formation" is called after the dominant species with the termination

*-etum, e.g. Callunetum, Pinetum,* etc. Celakovsky (1869) called by the same term, "formation," groups of species of similar habit, station, and distribution, which could be regarded as having migrated together into the country; he thus gave the word "formation" a meaning in floristic plant geography. Drude (1889) attempted to compromise by distinguishing the smaller "formations" as "Bestände," and Weber (1892) similarly classified them as primary and secondary "formations." The original definition of the term by Grisebach as a division of the landscape had lost its force from the newer biological way of regarding the "formation." It was now no longer strictly comparable to a geological formation, and could only in an indirect way be regarded as something "formed."

From the nature of the object sharp definition is impossible. Warming (1896) has recommended the disuse of the term; in its wide sense he says that its meaning can be quite well expressed by the word "Vegetation," and for groups of plants associated together by common habits he employs the term "Pflanzenverein" (in Danish "Plantesamfund"). In English we have named these unions or communities "Plant Associations," as expressing the nature of the relationship of the plants to each other. This expression is employed in a similar sense in papers by Flahault (1897 a), Schroeter (1894), Schneider (1897), etc.[1]

An association may be as narrow as that between two symbions, or as wide as the entire vegetation of the globe. In the description of the vegetation of any region it depends entirely upon how detailed a survey is intended, how many associations it may be subdivided into; if only a general survey, then only the chief and conspicuous associations in the landscape may be treated of, if a complete survey, then all down to the very smallest subdivisions await research. The complete knowledge of the "Vegetation" of any country demands such a complete survey. The methods organised for the study of "formations" apply equally well to "associations," since the "formations" selected are always particular instances of association.

These methods for the description of the "Vegetation" of a country can now be summarised. We require to know:—

(*a*) The chief associations into which it may be divided.
(*b*) The particular conditions of heat, light, moisture, and food distinguishing each association.
(*c*) The particular adaptations or life-forms of the species of each association.
(*d*) The relations between the species—
(1) Dominant social forms.

[1] One must point out, however, that the word "Pflanzenassociation" has sometimes been used by Loew (1879), Höck (1895), and some other authors with a special meaning in floristic plant geography, similar to the application of the term "formation" by Celakovsky as mentioned above.

(2) Secondary social forms struggling for dominance.
(3) Dependent species—simply protected by the presence of the dominant species, or living upon the humus they form, or parasitic upon them, etc.

(*e*) The influence of animals and of man upon *c* and *d*.
(*f*) The general conditions of climate and of vegetation of the region compared with other regions.

A concrete example of a brief survey of a type region will illustrate the application of these methods.[1] For this purpose I have chosen such an area as may be found in many places round the coast of Britain, namely, a stretch of sand dunes and links, the present example being from the Ayrshire coast, between Prestwick and Troon.

The small associations are numerous, but I select only the more important for the purposes of illustration. These are the strand plants, the dune bents, the grassy links, the heathy knolls, and the pine woods. If expressed in terms of the most important species of each, these would be respectively the associations dominated by *Atriplex*, *Ammophila*, *Agrostis*, *Calluna*, and *Pinus*.

The strand plants (*Atriplex*, *Cakile*, *Salsola*) are on loose shifting sand, occasionally submerged by spring-tides, and beaten upon by the wind. They are all annuals (cp. the great proportion of annuals to be found on the regularly overturned earth of cultivated land); recumbent in habit, thus spreading over the loose sand, holding it together and escaping the full force of the wind; succulent, an adaptation connected with presence of much salt; and their surfaces are reduced in extent or otherwise modified to prevent excessive transpiration. On the strand competition between the species is almost absent, for so few can live at all in these conditions that each plant has a free space of ground to itself. Human influence has not appreciably modified the conditions.

The dunes are beyond the reach of the waves, but are exposed to the fury of the wind, to drought, and to the moving of the sand. The dune plants are adapted in various ways to withstand excessive transpiration. Some are also salt-loving species, such as *Ammophila*, *Elymus*, *Agropyron junceum*, *Eryngium maritimum*, *Arenaria peploides*, *Volvulus Soldanella*, etc. The dominant and by far the most abundant species is *Ammophila arenaria*, which grows in great tussocks, holding the sand together, and sheltering from the full force of the wind numerous more delicate species that occur sparsely distributed on the dunes. The conditions are still too hard to allow a large flora, and competition between the species is almost at its minimum. Except

[1] For the present purpose, as will be clear, we are not concerned with how the plants came into this region, but with how they live there. The study of the immigrations of the British Flora has such an interest from its complex and striking nature that it has long attracted students. It is partly due to this that the oecological standpoint we are now assuming has received comparatively little attention.

in so far that a number of the casual plants in this association are weeds of cultivation, man has not modified it much.

The grassy links are on low-lying, more sheltered, and better irrigated parts, but still exposed to the sweep of the winds. The grasses (*Agrostis*, *Aira*, etc.) and herbs inhabiting the ground are perennials—low-growing, turf-forming, wiry species. Competition between the members is very severe, and very few plants can fight their way through the close turf which covers the ground. Sheep are pastured on the links, and rabbits in abundance nibble there. Only such species can exist that withstand this continual cropping. The manure from the animals enriches the soil, and here and there enables finer species of grass, clover, etc., to enter.

The association of *Calluna* is especially found on the dry, mossy knolls, where variations of water-supply must be considerable. The adaptations of *Calluna*, *Erica*, etc., to these severe conditions of life are well known.[1] This association is not particularly well developed upon the Ayrshire links compared with other similar stations where the association is protected and increased by being regularly burned by man. The immediate effect of burning is usually to allow a growth of grasses, *Vaccinium Myrtillus*, or other plants—secondary social species, which for a while dominate—but later the young heather comes up and peoples the area more fully than before.

The pine wood may be regarded as the natural arborescent vegetation of the fixed dunes or links, although here the present woods have certainly been planted by man. But if grazing animals are excluded, seedlings spring up naturally—as can be seen in some parts of those woods—so that when the wood becomes old, its vegetation is essentially that of a naturally-sown wood. Its situation includes ground similar to the grassy links and to the heathy knolls, and since these particular woods are fairly well lit, the subordinate vegetation is in part similar to that of these exposed stations. The pine association is complicated in its constitution. Sub-associations, dominated over by *Betula*, *Rubus*, *Ulex*, *Calluna*, various grasses, etc., occur in its clearings or under its canopy, according to the differences in degree of moisture, shade, or humus. Quite a hierarchy of forms can be grouped as constituting this association:—trees, shrubs, herbs, mosses, and thallophytes—each forming a different layer of vegetation. Certain forms characteristic of woods are present:—saprophytes and shade plants (*Goodyera repens*); climbing plants (*Lonicera periclymenum*); epiphytes (lichens and occasionally *Polypodium vulgare*), etc.

The classification of plant associations has recently been carefully considered by Warming (1896). He points out how the state of

[1] See Professor L. C. Miall, "A Yorkshire Moor," *Nature*, vol. lviii., August 1898, pp. 377-380 and 401-404.

moisture is on the whole the most important condition in determining any particular association, and he employs this as the chief basis of his classification. He groups the plant associations as follows:—

Hydrophyte vegetation.—This requires a substratum with at least 80 per cent of water. It includes associations of aquatic and marsh plants.

Xerophyte vegetation.—This is the opposite extreme from the previous class, requiring less than 10 per cent of water. It includes associations of rock, desert, moor plants, etc.

Halophyte vegetation.—Also an extreme vegetation, allied in many of its adaptations and habits to the Xerophyte vegetation, but characterised by requiring the presence of much salt, for instance the strand and the dune associations.

Mesophyte vegetation.—An intermediate class adapted to medium conditions of moisture, and not requiring a special amount of salt, for instance the associations of beech and oak woods.

For further subdivisions of these four classes we refer to Warming's original work.

In employing this classification it is essential to remember that variation in humidity is not the only acting condition,—the species are very differently adapted for the struggle with each other, even if the acting conditions are uniform; the nature of the food supply in the soil varies from place to place, and each variation may more or less affect the association; local conditions of light and shade, *e.g.* in the woods, modify the minor grouping of the species; and further, all the species are dependent upon the wider conditions of light and of temperature of the particular region of the globe. A similar physiological method of grouping plants to this of Warming was suggested by Alphonse de Candolle (1874), based upon the combined conditions of heat and moisture. Its application to the graphic representation of the vegetation of the globe will be seen in maps by Engler (1879) and Drude (1887).

The methods for the detailed mapping of the vegetation of a country have recently been greatly advanced by the labours of Professor Flahault of Montpellier (1897 b), who has shown the possibility of a very exact botanical survey, applicable to the consideration of questions of botanical geography, meteorology, geology, forestry, and agriculture. Briefly stated, the method consists in recording, by means of a large-scale contour map, the exact range over the country of certain representative plant associations. The selected associations are naturally as large and well defined as can be found; in this case, where Southern France has been studied, the dominant trees have

been chosen. Already a large district has been surveyed, and in 1897 the first sheet was published. It is the region of the Eastern Pyrenees, and shows clearly the areas in which the following trees respectively dominate:—evergreen oak, cork oak, ordinary oak, chestnut, beech, maritime pine, Austrian pine, Scots pine, silver fir, and mountain pine.

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# Mimetic Resemblances in Animals and Plants.

By Rev. Prof. George Henslow, M.A.

## *Insufficiency of Natural Selection.*

Mimetic resemblances in animals, either to others or to objects in their natural environments, are mostly regarded as protective; and this feature of protection has been assumed to be their rationale, or, in a sense, the "cause" of their existence. Darwinians assume that the animals which resemble their neighbours, by possessing a similar coloration and by adopting similar habits, are the result of the survival of the fittest, all others insufficiently resembling the protectors, *i.e.* all "unfavourable variations," having been killed off by natural selection.

Thus, Professor E. B. Poulton has lately published a paper on "Natural Selection, the Cause of Mimetic Resemblances and Common Warning Colours";[1] but in no part of it does he explain how natural selection can be "a cause." If he use the term metaphorically, it is misleading, for the other hypotheses mentioned in the paper do profess to supply actual physical causes, viz. "the direct effect" of the external conditions of life, or an "internal developmental cause." Natural selection is not "a cause" in any physical sense whatever. It only means, according to Darwin, "the preservation of favourable individual differences and variations, and the destruction of those which are injurious. This (he says) I have called Natural Selection, or the Survival of the Fittest."[2] On several occasions Professor Poulton discards all other hypotheses, and says, "Under the theory of natural selection the facts at once receive an explanation." Undoubtedly they may; *if only* its supposed powers were true and not imaginary. Natural Selection seems not unlike the wand of the fairy, which could evolve a coach and six out of a pumpkin and mice. But, in fact, all that it can tell us is, that some beings live and others die. It can neither account for the physical cause of a useful variation in one organism, nor say why another organism dies. It only, so to say, registers the fact.

[1] *Journ. Lin. Soc.* (Zool.) xxvi. 1898, p. 558.
[2] "Origin of Species," p. 63.

What, however, I wish to call attention to is, that mimetic resemblances cover a far wider field than that occupied by protective resemblances among insects. In fact, it is equally common in the vegetable kingdom, as it probably is in all classes of animals.

*Protection not always associated with Mimicry.*

Protection is no doubt a benefit, but it does not appear to be always associated with mimicry. It is questionable how far the effect is useful when all the animals of a district are coloured alike, as in a sandy desert; for not only are the carnivorous animals tawny coloured, but so are their prey, as may be seen in the collection in the entrance hall of the British Museum (Natural History Department), to which might be added a spider and *Helix desertorum* noticed by the present writer near Cairo.

It would seem more probable that there has been some general cause in the physical environment. The monochromatic light suggests itself as a cause acting upon all the creatures alike which are subjected to it; and if the result prove to be advantageous, so much the better for the creatures. That this is the right direction to look for a solution of such mimicry as superficial colouring, appears to be evident from the case of the house-mouse, which Mr. H. Lyster Jameson has described.[1] This author also speaks of "natural selection . . . weeding out unfavourable variations," but he does not appear to have met with any actual cases. He gives a list of thirty-six mice examined, but they range from the common domestic form to the palest-coloured of all. The variations, according to his description, are all in one direction towards a rufous or fulvous gray. In fact, they form a graduated series, but there is no mention of injurious varieties; indeed, after about one hundred years there are still some 14 per cent of the ordinary mouse-coloured forms still extant. And as the island is fully stocked (a female can give rise to nine at a birth), it is obvious that the sandy-coloured mice are as easily killed off as the others. The fact seems to be that as long as an animal coloured like the ground is *at rest* it cannot readily be seen, but when it *moves* there is little difficulty about it.

That natural selection cannot be a universal cause of mimetic coloration is obvious from the fact that some animals change from brown to white in winter, and many change according to the surroundings in which they happen to occur, as in the most familiar case of the chameleon, and a remarkable tick described by Sir J. D. Hooker, who says, speaking of a lizard—"Its throat was mottled with scales of brown and yellow. Three ticks had fastened on it, each of a size covering three or four scales: the first was yellow, corresponding

[1] "On a Probable Case of Protective Coloration in the House-Mouse (*Mus musculus*, L.)," *Journ. Linn. Soc.* (Zool.) xxvi. 1898, p. 465.

with the yellow colour of the animal's belly, where it lodged; the second brown, from the lizard's head; but the third, which was clinging to the parti-coloured scales of the neck, had its body parti-coloured, the hues corresponding with the individual scales which they covered. The adaptation of the two first specimens in colour to the parts to which they adhered is sufficiently remarkable, but the third case was most extraordinary." [1]

Without explaining how "the trick is done," the word "adaptation" seems best serviceable, both for mimetic coloration and mimetic forms. Thus, taking the Australian marsupials as examples, we have the well-known imitations of placental Mammals occurring among them as a result of similar habits, but certainly not of protection arising from similarity. Thus, Andrew Murray observes how "*Antechinus minutissimus*, the kangaroo mouse, closely mimics *Mus delicatulus*; the flying Marsupial *Petaurus* is a close counterpart in outward appearance of some of the flying squirrels. One or two of the *Phascogales* or *Antechini* resemble the jerboa; *Peragalea lagotis* has considerable resemblance to a hare, and carries its habits as well as its ears, making a form in the grass like it." [2]

That similar usages have educed similar structures, without affinity, is seen in cases where only certain parts of animals are mimetic. Thus the paddle of a whale resembles that of the *Ichthyosaurus*, and its tail that of a homocercal fish-tail, though it lies in a horizontal plane instead of a vertical one. Such resemblances are too numerous to mention; but there is no question of protective resemblances or of natural selection in the matter.

Inductive evidence of this kind brings us to the inevitable conclusion, not only that like causes have produced similar results, but that they are the direct outcome of the joint action of the outward physical conditions of life and the inward adaptive powers of protoplasm, coupled with the habits of life. Beyond that we do not seem capable of going.

### *Mimiery in Plants.*

Objections have been raised as to the applicability of the term "mimicry" to animals, and so *a fortiori* in regard to plants; for the word originally meant an actual imitation by a human being in gesture and manner. It might be conscious as in dumb-show, or unconscious as under delirium.

Now in mimetic insects the mimicry consists in the form and colour resembling others of no affinity, or in being like external objects, as well as in the adoption of similar habits. Thus, in the case of the "stick" insects, it is said that they allow their legs to assume unsymmetrical positions when at rest, so as to resemble a branch with

[1] "Himalayan Journals" (Minerva ed. p. 26).
[2] "The Geographical Distribution of Mammals," p. 53.

twigs more exactly. If so, there is both an unconsciously acquired resemblance in form and a conscious mimicry in action.

If, therefore, mimicry has been thus allowed to have its meaning extended to include a superficial or structural resemblance in animals, often apart from imitation in habit, we are quite justified in applying it to plants, among which such resemblances abound.

In the vegetable kingdom it arises from common causes and issues in common uses.

Mimetic resemblances are particularly characteristic of plants growing in very specialised environments, such as under water; in high alpine, arctic, or antarctic regions; in deserts and arid countries, etc.; as well as of plants which are highly specialised for carrying on certain peculiar functions, as of insectivorous, climbing, parasitic plants, etc.

In all these the same functions have resulted respectively in producing similar structures. In other words, whether one studies the external mimetic morphology or internal anatomy, the conclusion appears to be inevitable that these imitative features are simply due to the direct action of the environment, together with the responsive powers of protoplasm; though one cannot explain how the process is effected.

Aquatic Types.—Commencing with submerged leaves, the type of foliage most common in dicotyledons is finely dissected, though as soon as the stem reaches the surface of the water it may develop more or less lobed or entire leaves, as may be seen in *Ranunculus heterophyllus* and the water-lily.

Now this type is imitated by many aquatic plants of no affinity, as in *Cabomba* (Nymphaeaceae), *Myriophyllum* (Halorageae), *Hottonia* (Primulaceae), *Apium inundatum* (Umbelliferae), *Ceratophyllum* (Ceratophylleae), etc. Affinity among these plants is quite out of the question; but since the water crowfoot is undoubtedly descended from some terrestrial buttercup, and the Halorageae are aquatic forms of Onagraceae, and *Hottonia* is allied to land primroses, etc., the inductive evidence is ample to prove that this type of submerged foliage is entirely due to the direct and arresting action of the aquatic medium, which brings about degradations throughout the entire plant.

Another type of submerged leaf is ribbon-like. This is seen in *Lobelia dortmanni*, *Hippuris*, etc., but it is commoner among monocotyledons. From these, as in *Sagittaria*, we learn that this form is really phyllodinous as long as it is in deep water; but when the surface is reached the leaf develops a blade at the summit. And since a progress from phyllodes to sagittate blades is imitated in Nymphaeaceae, we again see a direct cause and effect.[1]

Juncaceous Type.—A stem with much reduced leaves and a rounded

[1] For fuller details of these and the following types the reader is referred to "The Origin of Plant Structures," Internat. Sci. Series, vol. lxxvii.

rachis, resembling those of a rush, is characteristic of many marsh plants, but is not confined to the genus *Juncus*. Thus *Oenanthe fistulosa* and *Crantzia lineata* (Umbelliferae), several Cassias, *Spartium junceum*, etc. (Leguminosae), *Scorzonera*, *Enhydra* (Compositae), *Convolvulus chondrilloides* (Convolvulaceae), *Johnsonia* and *Sowerbia* (Liliaceae), *Bobartia* and *Lansbergia* (Irideae), are all good illustrations enforcing the same conclusion as to the common origin of their common resemblances.

Xerophilous Types.—One of the commonest mimetic features of plants growing in very arid districts is to be spiny. Our own *Genista anglica* (Needle-Furze) and *Ulex europaeus* (the Common Furze) will illustrate it; but "thorns and thistles" have been recognised as typical of deserts from the earliest ages.

That drought is the actual cause of spinescence in all cases has been easily proved by growing such plants in a moist soil and air, when the spinescence rapidly disappears.

Another xerophilous and more remarkable type is the fleshy stem. Thus the Cactaceae of Mexico mimic very closely the Euphorbias with thick leafless stems of the Soudan, as well as the Stapelias (Asclepiadaceae) of South Africa. As the conditions of life in which these three groups live are the same, their peculiar vegetative structures have been similarly evolved.

Alpine Types.—One of the most characteristic types of vegetation is that of the Cypress, *Thuia*, and Juniper, the leaves of which are extremely minute and closely adpressed and imbricated along the slender shoots.

Now, we find this type of foliage exactly paralleled in, or mimicked by Alpine species of the genus *Veronica*, which abound in New Zealand. The species of the lowlands are shrubby, with large leaves; those of a higher elevation begin (say) with *V. buxifolia*, so called because its leaves resemble those of the box; then on ascending to 4000 and 5500 feet there occurs *V. cupressoides*, the specific name indicating its mimetic resemblance to the Cypress; while at 8000 feet *V. lycopodioides* is found, resembling the *Lycopodium*, or Club-Moss.

As drought appears to be one of the direct causes of this diminished type of foliage, it is not surprising to find it mimicked in the African deserts by *Salsola pachoi*, etc. So, too, in the Antarctic regions, it is exactly paralleled by that of *Drapetes muscosa* (Thymelaceae), *Bolax glebaria* (Umbelliferae), *Lyallia kerguellensis* (Caryophylleae), *Forstera glavigera* (Stylideae).

Muscoidal Type.—This is one of the extreme forms of high Alpine as well as of Arctic and Antarctic forms. Besides the above-named species of *Veronica*, there are *Saxifraga bryoides*, *S. muscoides*, *Cherleria sedoides*, *Silene acaulis*, etc. Lastly, *Bolax glebaria* (Umbelliferae), of the Falkland Islands, is mimicked by species of *Acanthophyllum* (Caryophylleae), in Afghanistan, and by *Haastia* (Compositae), in New Zealand.

Grass-Leaved Type.—Herbaceous plants growing thickly together, and so preventing each other from spreading out their blades horizontally, have often assumed a grass-like foliage. Hence a caespitose habit has induced a vertical position in the foliage, so that the leaf has acquired a narrow linear form, the anatomical details following suit by its developing stomata, etc., on both sides. *Lathyrus nissolia*, which grows among grass, has this type of leaf. Thrift, Pinks and Carnations, Sedges and the Bog Asphodel, as well as Grasses, have similarly-formed leaves. Those belonging to dicotyledons retain the reticulated venation, the branches starting from near the base at an acute angle, thus more or less imitating the parallel venation of the monocotyledons.

Specialised Mimetic Organs. — Leafless but foliaceous stems, closely mimicking true leaves, occur in widely-separated orders having no affinity. Thus *Xylophylla* (Euphorbiaceae) may be compared with *Ruscus* (Liliaceae); while of leafless but winged stems the following are good examples:—*Bossioea scolopendria*, *Genista sagittalis*, and species of *Acacia* (Leguminosae).

Climbing Plants.—Mimicry is well seen among these; as, for example, between the tendrils of a vine or passion-flower, which consist of metamorphosed flowering branches, and those of a pea, which are homologous with a leaf. In *Dissochaeta*, aerial roots undertake the same function, as do the slender branches of species of *Strychnos*.

Ascidiform Type.—Perhaps no better illustration of plant mimicry could be given than that between the pitcher of *Cephalotus follicularis* and that of species of *Nepenthes*. These two genera are monotypic, and therefore imply a long and now lost ancestry. The general appearance of the mimetic pitchers is precisely the same in both. There is a similar "lid," a pitcher of the same shape, with an inrolled margin, glands sunk into the surface of the lining of the pitcher, which externally carries a fringed guide from bottom to top. Yet, while the pitcher of *Cephalotus* is a metamorphosed leaf-blade, that of *Nepenthes* is developed out of a water-gland situated at the apex of the blade, this latter taking no part in its formation whatever.

Such complete mimicry as this is quite as astounding as any between two insects, whose forms and colours are alike, or between the kangaroo mouse and the genus *Mus*.

Conclusion as to Mimetic Vegetative Organs. — The above-mentioned cases are but samples of what may be called a general principle in nature; which is, that since the living protoplasm is of one and the same kind in all beings, as far as we know, where a certain feature is evolved, a similar one may be expected under similar conditions, and a mimetic organ is the result; that is, so far as the conditions of the structure will allow. Thus as the kangaroo mouse mimics a true mouse, but retains its pouch and insectivorous teeth; so a linear dicotyledonous leaf imitates a grass blade, but retains its branching venation.

Mimetic Flowers.—These are by no means uncommon. Bracts are often coloured, and so to say answer for a corolla, as in *Poinsettia*, *Euphorbia*, some Umbelliferae, and "everlastings." But the mimicry may be much more exact, and so close, indeed, as to deceive the unwary. Thus the four white bracts of species of *Cornus* render the inflorescence exactly like a flower of a *Clematis*. *Darwinia tulipifera*, as the name implies, is very like a tulip; while *Euphorbia jacquiniaefora* has five scarlet lobes on the rim of the involucral cup, thereby mimicking a corolla with five petals.

The papilionaceous corolla of the Leguminosae is imitated by the so-called "falsely-papilionaceous" corolla of *Polygala*. It is also seen in the gamopetalous corolla of *Collinsia bicolor* (Scrophularineae), the front petal of which closes over the stamens and pistil precisely as do the keel petals of *Genista* or other member of the Leguminosae. A similarly shaped corolla is seen in some species of *Pelargonium* (Hoarea group). Again, a spike of flowers of an orchid, *Disa cooperi*, is very like one of a larkspur with its upturned and elongated spur.

Lastly, *Crocus* (Irideae), *Sternbergia* (Amaryllideae), and *Colchicum* (Liliaceae) have precisely similar perianths, though representatives of as many orders.

General Conclusion.—When all the above facts (and many more might have been given) are considered together, and when it is noted that in many cases experimental verification shows that the peculiarities in question are the results of the definite or direct action of the environment, the inductive evidence is overwhelming that mimetic results are in all cases the consequence of the environment influencing the protoplasm to adaptive response. It will thus be seen that natural selection is quite uncalled for, and, in fact, has no *raison d'être* in the origin of any structure whatever.

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# Bees and the Origin of Flowers.

By G. W. Bulman, M.A., B.Sc.

Those engaged in the study of the mutual relations of insects and flowers will read with interest a short paper in *Natural Science* for October last on "Bees and the Development of Flowers," by Mr. F. W. Headley, Haileybury College, New Zealand. It is to be feared, however, that this writer's attempt to save the situation for Darwinism as regards the origin of flowers will hardly stand the strain of criticism. Mr. Headley admits that the habits of bees in passing freely from variety to variety, or from one nearly allied species to another, will tend rather to retard than promote the development of new species. "Thus it is just where her constancy might seem most needed," he writes, "that it breaks down. When new varieties are arising, the operation of bees comes in to swamp them, if possible, by intercrossing, and so prevent them from developing into species." This, indeed, must be obvious to all. But while giving up the old explanation, Mr. Headley still thinks the colours of flowers must be explicable on Darwinian principles. "*A priori*," he says, "if it be granted that the Darwinian hypothesis affords a satisfactory explanation of other phenomena of the animal and vegetable worlds, it seems unlikely that it should leave the colours of flowers unexplained." This is doubtless true, and if the origin of species is due to the Darwinian principle of natural selection, then the colours of flowers must be due to the same. To stop the gap, then, caused in the theory of the origin of flowers by insect agency by the loss of the constancy of the bee, Mr. Headley takes a plank from Dr. Romanes' doctrine of physiological selection. The action of the bee, he thinks, in carrying pollen from variety to variety will be of no consequence, since these varieties are sterile *inter se;* while in carrying it from individual to individual of the same species it will still give an advantage to those it visits. As proof of this sterility between varieties of wild-flowers the experiments of the French botanist, Alexis Jordan, quoted by Romanes in "Darwin and after Darwin," vol. iii., are referred to. But even admitting that Jordan proved his case, we have still the following objections to bring forward. If the bee visits indifferently *all* the varieties of a

species it cannot raise one or more of these to the rank of distinct species at the expense of the others, as it ought to do on the principles of natural selection. Again, admitting that all species, sub-species, and varieties as they exist to-day are sterile *inter se*, we cannot suppose that the *varying individuals* in a species—which must form the beginning of a new species—are so. And it is with such that the bee has to deal at the beginning of the development. *Ce n'est que le premier pas qui coûte*, but if you cannot take the first step it does not matter how easy the rest are. Thus the principle of physiological selection will scarcely man the breach caused by the loss of the constancy of the bee. But can we accept fully the principle that varieties are as a rule sterile *inter se?* Dr. Romanes, indeed, seems to have accepted it unreservedly, but it is hard to resist the conviction that had it been less in accordance with his theory of physiological selection he would have subjected the evidence to more rigid criticism. The experiments, we may note, were made some twenty-five years ago, and do not appear to have been since confirmed. They were also avowedly made for the purpose of proving that each species and variety had been specially created. Darwin, moreover, found that crosses between slight varieties were more, and not less, fertile than those between individuals of the same variety. This is also the experience of gardeners and breeders.

With regard to the question of the influence of colour on insects, Mr. Headley writes as follows:—

"To account, then, for the colours of flowers we have the proved colour-sense of bees. Sir John Lubbock has tested it by experiment: they are attracted by brilliant blossoms, and, therefore, it has been to the interest of plants, in order to obtain cross-fertilisation, to produce conspicuous flowers."

But whoever ventures to form an opinion on this very difficult subject will have also to take account of the exhaustive experiments recently carried out by Professor Plateau of Ghent. That gentleman concludes, as the result of his researches, that insects are attracted to flowers in a very subordinate way by sight, and chiefly by another sense, viz. smell. My own observations do not lead me to go so far as to say that the colours of flowers have nothing to do with attracting insects to them. But I do most emphatically assert that they are indifferent as to what particular colour the flower they visit is. As far as I am able to judge, it matters not one iota to a bee whether the flower is blue, red, pink, yellow, white, or green: so long as there is honey, that is sufficient. Indeed, we sometimes hear of bees gathering the honey-dew from the leaves of trees, visiting sugar refineries, and robbing jam factories.

As regards the supposed constancy of bees in visiting flowers Mr. Headley expresses his opinion thus:—

"That they show a remarkable degree of constancy is undeniable.

When at work upon dandelions they will not wander to a neighbouring narcissus. In thus keeping to flowers of the same make, they are consulting their own interest."

Now, while in this and in other parts of his paper Mr. Headley goes farther than is allowed by the advocates of the insect-selection theory, and too far to render that theory tenable, it is still, according to my observations, short of the truth. Thus, while I have never seen a bee go from dandelion to narcissus I have seen them go from dandelion to chickweed, from Iceland poppy to Cistus, from herb Robert to lavender, from snapdragon to woundwort, from devil's-bit scabious to knapweed, etc. The fact, however, that bees pass at times from one very different species to another, while it renders the bee still more impossible as an evolver of flowers on the old view, does not affect Mr. Headley's proposed amendment.

Another point in Mr. Headley's paper seems to call for remark. He asserts that flowers not fertilised by insects "are almost all of them dull and inconspicuous." But there are probably a good many exceptions to this rule. One of these is specially mentioned, viz. the larch, of which it is said, "In these the colour may, possibly, be looked upon as a by-product of the physiological activity of the plant. The more striking blossoms, elaborate in form and coloration, cannot possibly be mere by-products."

But this is no explanation: it might with equal truth and justice be said of the blue of the larkspur, the crimson of the cranesbill, or the yellow of the buttercup. Indeed, in every flower the colour is the product—why in any case call it a by-product?—of the physiological activity of the plant. If the blue of the hyacinth and harebell require a further explanation, so does the red of the larch.

In conclusion, a word as to my own position as regards this question. Mr. Headley remarks that certain naturalists, including myself, "have come to the conclusion that the colours of flowers have arisen quite independently of insects." I should prefer to define my position in slightly different words. Instead of saying I have come to the conclusion that the colours of flowers have arisen quite independently of insects, I should like to put it thus:—

I conclude, as the result of my observations of the habits of insects visiting flowers, that the theory of the origin of the latter by their selective action, as taught by Darwin, Wallace, Hermann Müller, Sir John Lubbock, and Mr. Grant Allen, is absolutely incompatible with the facts of the mutual relations of insects and flowers.

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# Principles of Animal Development.

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## I. *A Whelk's Egg.*

"Omne vivum ex ovo" is now an ancient aphorism, and it has become a commonplace to say that the starting-point of almost all animal development at the present time is the fertilised egg. The phenomena leading up to this need not concern us at present. Let us assume that we have got our fertilised egg, and let us in the first place consider what sort of a thing it is. If the choice of such an egg be given, how great is the variety out of which it may be selected. It may be the microscopically small egg of an Echinoderm, or one of larger size from the cocoon of an earthworm, the still bigger one from the leathery egg-case of a whelk, or the huge one from the marvellous egg-capsule of a smooth skate. It may be so large and contain so much food-material that the developing organism may feed upon it for two years, or so small and so destitute of nourishing substances that it can suffice for the needs of the development for but a few hours.

It may be destitute of any envelope beyond a thin structureless membrane formed by the egg-cell itself, or it may be closely wrapped up in a series of coverings, and outside of these we may find a shell or case of complex form and architecture.

The eggs of animals are, indeed, endless in variety, when the size, amount of food-yolk, coverings and appendages, and the modes in which they are deposited are taken account of. But with all this one fact stands out quite clearly, that the size, composition, mode of deposition, etc., of any particular egg, have an intimate relation and connection with the development of an individual of the species whose egg it is.

The individual peculiarities of ova raise various interesting questions which have hitherto received but the barest consideration in embryological text-books and even in developmental researches. Probably for solutions of most of them the science will have long to wait. The nature of most modern embryological research is, unfortunately, not such as to furnish hints in the direction of their elucidation;

the amount one man can accomplish, even if he be conscious of the existence and importance of the problems, is but the merest trifle, and his labours would require an enormous comparative material, if the results were to be of far-reaching import.

These reasons, paradoxical though it may seem to be, impel the writer to attempt some sort of consideration of the subject. It might be made the work of a lifetime without evidence of exhaustion of the subject. If we are ever to have a true and real comparative embryology of organisms, as well as the existing comparative embryology of organs, many questions relating to the eggs of animals will need to be cleared up.

Facts are to be found for the seeking; but, as every conscientious embryologist realises, the search nowadays, especially if it be in some definite direction, is often long and arduous. And the result may often be a single fact and a dozen new problems. Moreover, most of us have our plans of work mapped out for years to come, and have no desire to forsake the plot of ground which we have diligently and hopefully tilled, before we have reaped our little harvest. Therefore, since the problem of the individuality of eggs with which we have begun forms no integral part of our personal task, what we have to say under this head partakes rather of the nature of incidental gleanings and musings than of deliberate investigation. We have attempted to enunciate some of the problems without pretending to do more than suggest what may be the nature of some of the solutions.

Examine on the sea-shore the egg-capsules of a dog-whelk (*Purpura lapillus*). If one of the freshly-deposited cases be opened, in its interior a large number of minute eggs may be counted. Some time later visit the same spot and open others belonging to the same bunch. The probability is that the enumeration of the developing organisms in all the egg-cases of the bunch will not give as large a number as that already recorded in the single freshly-deposited one.

This is of course an old story. The cannibalism of the developing young of the dog-whelk and whelk is one of the commonplaces of marine zoology.

Many years ago the writer endeavoured to study it for himself, but as the examination of the cases was begun, when, as it turned out subsequently, the eliminative process was over, it was naturally not observed; although the facts were looked for until the young whelks emerged from their cases. The process on that occasion was not seen, simply because it happens at a very early period. One interesting little point did, however, reveal itself, that the number of young whelks within an egg-case was fairly constant, about five or six. If the process be mere cannibalism, it is not easy to perceive why it should stop short, when some five or six larvae were left in the egg-case, why, as almost always happens in the case of the Alpine *Salamandra atra*, one should not devour the rest.

The number is so constant in the whelk, that one may suspect that it is governed by some law. Probably, too, if one counted them, the number destined to be devoured would be found to be equally constant. The whole process must take place under the workings of a law or laws, and it is part of our task as embryologists to try to find out what these are and what determines them.

If, after the examination of the whelk-cases, a fully-formed egg-purse be taken from the oviduct of a dog-fish or skate, on opening this we shall almost certainly find but a single egg.

The eggs of different species of dog-fish or skate also differ considerably in size, both as regards the egg-cases and the yolks. These differences are not always in relation to the specific differences in size. The smooth skate (*R. batis*) is much larger than the starry ray (*R. radiata*), but no proportion can be detected between the sizes of the two skate and those of their eggs. Indeed, if we study the ripe eggs of two closely allied species of shark, *Mustelus vulgaris* and *M. laevis*, we find that, although the two fishes are almost of identical size and so alike that no fisherman could ever be expected to distinguish between them, their eggs exhibit great differences in size along with almost inappreciable differences in the texture of the egg-capsule.

What, then, are the essential differences between the eggs of these two species? The egg-shell of *M. vulgaris* appears to be rather thicker than that of *M. laevis*, and—a very important point—the actual egg, the yolk, of the former is the equivalent in size and weight of about four of the latter.

This difference cannot be ascribed to the relative sizes of the two species, and, as a matter of fact, it is in association with a very fundamental difference in the mode of development.

Both species are viviparous, and it is possible that the young are born in both cases in the same condition of development, though this has yet to be determined. In *M. vulgaris* the yolk suffices for the whole of the uterine development, whereas in *M. laevis* it is used up long before uterine life is over, and for what is probably a long portion of its uterine existence the young *M. laevis* is nourished by a sort of placental attachment of its yolk-sac to the uterine wall, as recorded by Aristotle, and as rediscovered during the present century by that great embryologist, Johannes Müller.

The instances above mentioned may serve as the text for further study. Modern embryology has never yet seen any difficulties or even any problems in the matter at all. We have our classifications of various kinds of eggs. We recognise eggs with little or no food-yolk, and those with much. Various forms of segmentation or cleavage of the egg are distinguished, such as equal or adequal, unequal but complete, and meroblastic or partial. Our leading text-books either say, or lead the reader to infer, that the differences between the various forms of cleavage are dependent upon the amount

of food-yolk. As compared with the egg of a frog or toad that of a cod-fish contains but little food-yolk, yet the former exhibits unequal but complete cleavage, and the latter partial or meroblastic. Our classification may be convenient for purposes of elementary instruction, but it is purely empirical and has no real scientific basis.

It is like everything else. With the advance of knowledge, our conceptions of organic Nature become enlarged, and we come more and more to perceive how adverse Nature is to schematic and empirical classifications. But within recent years we have gone further still in the use or abuse of food-yolk in embryological science. It has even been made the basis of a phylogenetic tree of vertebrate ancestry. What more could be asked of it than this? The discoverer of this tree has indeed disowned and rejected the figment of his own imagination; but, this notwithstanding, from time to time it tries to blossom forth anew.

If it could be proved that, as we ascend the vertebrate scale, the food-yolk either increased or decreased in a regular and intelligible fashion, good reason might underlie its use in phylogenetic speculation. But if we attempt to evolve dog-fish and skate from lampreys by increasing the amount of food-yolk in the egg, ganoids from the former by again reducing it, and amphibians from these by a still further reduction, and by a new and enormous increase reptiles from amphibians, and so on to the end of the mammalian chapter, we are not really drawing upon an actual tangible, but limited, supply of food-yolk, but simply and solely on the intangible and unlimited resources of the imagination.

If it be suspected that the food-yolk of an egg has either increased or decreased, some cause must have been behind the change. It is only in fairy tales, such as that of "Aladdin and the Wonderful Lamp," that things come into existence from nowhere and out of nothing. As in physical science so also in natural science every effect has a cause and every effect is governed by a law or by laws.

If the food-yolk of an egg can be shown to have increased in amount, we must not forget that the egg has had a history,[1] in past times, and that a plus added to it at or during some epoch of time necessarily entails a minus or abstraction from something else at the same time.

That this must be so may not be obvious at the first glance. But it is all but certain that the cells in the ovary which feed the ovarian egg (follicle cells), or in other cases the ovarian cells consumed by the ovarian egg, are themselves rudimentary or degenerate or sterile eggs. And the amount of nourishment they can furnish in any given instance would seem to be definite and limited. If the egg of an animal increase in size and amount of food-yolk it may in all probability

[1] This might be termed a "phylogenetic history," were it not liable to be misconstrued into confession of faith in recapitulation.

be rightly concluded that fewer eggs have been laid, and that in some form or other more incipient eggs have been used up in the process of forming the functional one.

This conclusion, or suspicion, brings us naturally to the main points of our inquiry, *i.e.* to the modes in which food-yolk has been obtained, and an egg-capsule for the reception of a single egg evolved.

The simplest eggs are admittedly those, like the eggs of many Echinoderms, which contain little or no food-yolk, possess only a single membrane formed by the egg itself, and are laid singly, an egg-capsule being entirely absent.

It is always found that such simple eggs are laid in great numbers, for few or none have been rendered sterile, or degraded to serve as food to the others.

Naturally in such cases the developing organism can attain no great degree of complexity of structure before its original source of food-supply, that contained in the egg itself, is used up. The organism must then hatch out and seek food for itself from extraneous sources. Under these conditions the resulting organism possesses at its birth the simplest possible structure, that of a gastrula composed of two layers of cells and with an aperture leading into the gut.

Apparently there are two ways in which a further supply of food might be bestowed on the developing egg. Some of its fellows might be rendered sterile in the ovary, and there be used up, as in liver-flukes, tapeworms, insects, etc., to increase the food-supply of the remaining functional eggs. Or, a big batch of eggs being deposited together within a simple structureless cocoon, as in earth-worms, leeches, the lug-worm, and the whelk, some might be devoured by others to form a reserve supply of food, as is the case in the whelk.

Whether there is any connection between the two processes is a difficult question to decide. It may be that they have been separately evolved and that they have proceeded along parallel lines in their subsequent history. On the other hand, in some cases, at any rate, it is possible that the latter of the two processes was the original one, and that in course of time it has passed into the former.

If the indications are wanting, that the eggs of lampreys, ganoids, and bony fishes ever passed through the latter condition of being deposited in batches in simple cocoons, the evolution of a skate's egg and purse is only intelligible on this supposition.

Taking the latter process first of all, it will be noticed that it ultimately leads to two results,—to the formation of a large-yolked egg and to the evolution of a complicated egg-capsule for its reception.

In its very beginning the formation of a simple cocoon entails the co-operation of structures external to the ovary in the shape of genital ducts. These are necessary in order that a cocoon, however simple in form and structure, may be produced. It is the entire absence of any such structure in connection with the eggs of lampreys, bony fishes,

etc., which would appear to point to a different history of the yolk in these cases. Reverting to the cocoon, a very simple structure, formed by a genital duct, and containing a great number of small eggs with little food-yolk, may be looked upon as the beginning of the process. The lug-worm, *Arenicola*, furnishes an example. Larvae are developed within the cocoon, and live there as long as they have food to consume. If they then find no other nutriment within this structure they must emerge, and seek for food elsewhere.

Further life within the cocoon is only possible on one or other of two conditions. The substance of the cocoon itself, or a part of it, *i.e.* all except its cortex, may be of a nutritive value. This is so in certain leeches, for instance. Here the larvae, as soon as they reach the gastrula condition, proceed to gorge themselves with the semi-fluid "albumen" of the cocoon, and this food-supply suffices them until the young leech is formed.

But in some animals with simple cocoons the contents of the cocoon apart from the eggs appear to possess no nutritive properties. Undoubtedly it is to be looked upon as a secondary condition, when the gelatinous "white" of the cocoon acquires a food value.

A parallel to this is seen in certain eggs of vertebrate animals. The egg-white of an Elasmobranch egg contains only the merest traces of albumen (Johannes Müller), and is of no use as food. Whereas the same egg-white in a bird's egg is largely albuminous and very nutritive.

But in the simple cocoon, although the substance of this structure is sometimes of no value as a food, there may be unfertilised or undeveloped eggs, or abnormal or degenerated larvae, and these may serve as food for the normal ones. Thus, in the second way, the "birth-period" may be postponed.

There are well-known cases among the Mollusca, *Purpura* and *Buccinum*, in which this "cannibalism" is the normal course of events. A few of the eggs in a cocoon develop quickly, and become gastrulae, with large mouths and muscular gullets. These few very soon use up their own food-supply, and then proceed to devour their fellows, which have either not developed at all, or only gone a little way. If this were carried to an extreme—and it is probable that such cases do occur—it would end in there being only one developing organism left in the cocoon, and this would be gorged with the food-yolk obtained by the annexation of its fellows.

The original cocoon by the solidification of its outer walls, its centre remaining semi-fluid, might thus become an egg-case for a single egg. This would be realised in fact if the eggs to be consumed, instead of being deposited as separate entities within the cocoon, were joined to or devoured by that single one which was destined to develop while in the ovary. The well-known instances of *Hydra* and *Tubularia*, with their ovarian "cannibalism," are obviously illustrative.

When this transference to the ovary of the process of devouring took place, is of course a moot question, but it must have happened at some time or other in many cases. The originally large number of eggs in the cocoon then becomes reduced to a single large one by the conversion of the rest into yolk-material in the ovary itself. And thus, having at first served a rather different purpose, the cocoon becomes an egg-case for a single egg.

Though such an egg contains a large amount of food-yolk in many instances, this cannot be retained during cleavage and the subsequent processes within the cells of the developing organism, and these act as if they were not connected with such an enormous mass of food. The egg now, as in a skate, segments in a disc-like fashion, or is "meroblastic." The object of the developing organism now seems to be to regain possession of the food-mass, which it had been obliged to relinquish during cleavage. This it does by forming once more a gastrula. The latter is flattened out on the top of the mass of yolk, and in order once more to annex this yolk, the gastrula must grow round and enclose it.

So that, if the mass of yolk be large, it appears to be a matter of indifference whether it be added to or devoured by the ovarian egg, or whether it be first obtained by the swallowing of other eggs within the cocoon or egg-case. For the former case ultimately resolves itself in a mere modification of the latter. To put it in another way, the devouring of the yolk-mass has in both cases to be undertaken by the developing organism before it can be said to have obtained real possession of the yolk.

As I have elsewhere written,[1] it is thus that the growth of a blastoderm, *i.e.* of a flattened gastrula over a yolk-sac, is exactly comparable to the devouring of yolk-masses by a whelk-gastrula. The end is the same, and probably the beginning was the same in both.

We have now got but a little distance along the path of evolution of animal eggs, and other interesting problems remain for examination. On another occasion the study of the purse of a dog-fish or skate may engage our attention. From this we may pass to the consideration of the egg of a mammal, and finally, this should lead to the elucidation of some of the principles underlying mammalian development. Food-yolk will here again be found to form an integral part of the programme, and we may perhaps come to realise with what nicety Nature bestows it on animal eggs. And if we glean nothing else, we may at least learn to realise that the reign of law pervades animal development from its commencement to its close.

[1] "The Span of Gestation," Jena, Gustav Fischer, 1897.

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# The Colours and Pigments of Butterflies.

By M. I. Newbigin, D.Sc.

In a previous issue of *Natural Science*, in the course of a paper on "The Pigments of Animals," an account of the general colour-phenomena of the Lepidoptera was given, and the great interest and importance of the subject indicated. Since the publication of that paper, not a few observers have been engaged in pursuing further investigations, and numerous additional papers have appeared; some of these it is proposed to briefly summarise here. For a somewhat detailed account of the papers published up to the end of last year, reference may be made to the chapters on the subject in my "Colour in Nature" (1898), but preparatory to the consideration of more recent work, the results of these earlier observations may be very briefly stated here.

The researches of Hopkins, Spuler, Urech, Mayer, and many others have shown—(1) that the colours of the wings in butterflies are due sometimes to pigments, sometimes to optical effects, and sometimes to a combination of the two; (2) that pigmental colours are shades of yellow, red, deep brown, or rarely white, green, and pure black; (3) that the white, yellow, and red pigments of the Pieridae, and possibly of other butterflies, are members of the uric acid group; (4) that the red, yellow, and white pigments at least in the Pieridae are chemically nearly related, while the black and brown pigments differ markedly from them, and are of unknown chemical relations. Further, it has been noticed by several different observers that, in the development of colour in the pupa, white or yellow tints first appear, and there is then a gradual darkening of tint as development proceeds—black always being the last tint to appear.

Of the more recent papers with which we are concerned here, we shall take first one by Dr. M. Baer on the minute structure of the wing-scales in diurnal Lepidoptera. In its general outline, the paper for the most part merely confirms the results of others, but on several points it is more detailed, and it is valuable as a clear statement of the general characters of the colours of butterflies and their causes. The value of the paper is greatly increased by the fact that the author did not confine his observations to one family, and therefore is enabled to

clearly point out the differences between the Pieridae and other butterflies.

As regards the optical colours of butterflies, Baer, like Spuler, finds that the particular tint may be due to the sculpturing of the individual scales, or to the combination of different scales. Scales displaying optical colours usually contain pigment in addition, but this pigment does not produce any marked effect on the colour, although it is no doubt a factor in its production. Other apparently optical colours, such as emerald-green, peacock-blue, violet, etc., are, according to the author, produced by the superposition of scales containing pigment, and scales displaying optical colours. The exact physical cause of the optical colours was not ascertained.

Of more importance for our immediate purpose is the treatment of pigmental colours. Histologically, the pigments were found to be of two types—diffused and granular. Pigments of the former type occur diffused through the chitin of the scale, are usually present in very small amount, and include the dark pigments, most yellow, orange, and red pigments except in the Pieridae, and the white pigment (uric acid) of the Pieridae. Contrary to the results of Hopkins, Baer finds that some of the orange and yellow pigments of the Pieridae are diffuse and not granular. Granular pigments occur exclusively in the Pieridae, and are yellow or red in colour. They colour the scales in which they occur very deeply, and associated with this fact Baer finds that such scales are few in number, and almost without surface sculpturing. The superposition of dark-coloured scales upon scales deeply tinted by yellow granular pigment may, as in *Anthocharis cardamines*, produce a greenish tint.

Another recent paper by the Countess Maria von Linden is chiefly concerned with the development of the markings of the wings of butterflies, but incidentally also discusses the order of development of the colours. The author, like preceding observers, finds that those parts of the wing in the pupa which are ultimately black, are first colourless, then yellow or yellowish-white, then orange and orange-red, and finally attain their black tint. The last stage, the development of the dark tint, is not due to a replacement of the lighter pigments, but to a superposition of darkly tinted scales over light ones. Optical colours, such as blue, appear later than the dark tints, and are only exhibited by scales which contain dark pigment. Though the paper is not primarily concerned with pigment, it nevertheless is of importance as tending to emphasise the distinction between the dark pigments and the others, and the late appearance in ontogeny both of these dark pigments and of the optical colours.

The two preceding papers may serve as an introduction to a third, a much more elaborate treatise, by Herr M. C. Piepers, on "Colour-evolution in the Pieridae." Under this title the author discusses a variety of topics connected with the colours of butterflies

and colour phenomena in general; but the paper differs from the preceding in that it is concerned with the broad interpretation of facts observed in the field, rather than with the setting forth of the results of special research. As readers of his previous papers are aware, the author speaks with the authority of one having large experience of butterfly-collecting, especially in the Far East. Like many others, he has as a collector been greatly impressed by the phenomena of sexual dimorphism and polymorphism, and colour variation in general; but, unlike many field naturalists, he is unable to accept the suggestion that these are due to secondary causes. On the contrary, he believes that they prove that the colours of butterflies tend to vary in a definite direction; that there is an evolution of colour. In other words, he holds that if certain genera in a given family include species which exhibit colour varieties, then, in the different genera, the colour varieties will be of similar nature, certain colours being in each case characteristic of the less specialised species or varieties, and other colours characteristic of the more specialised forms.

Beginning with this hypothesis, the author seeks, by a series of examples, to determine the course of colour evolution in the Pieridae, and to consider the bearing of his results upon various problems.

By a detailed study of various genera, notably *Tachyris* and *Hebomoia*, he shows (1) that the amount of red in the wings varies greatly; (2) that red and black vary inversely in amount; (3) that in dimorphic species the male has usually more red and less black than the female; (4) that where the fore wings contain much red, yellow occurs on the hind wings, but where red is present in small amount, yellow is replaced by white. To put this briefly, red and yellow on the one hand stand in antithesis to black and white on the other, and red tends to predominate in the males. Further, the conditions under which red occurs, sometimes as a ground colour, sometimes as spots of varying size, together with its late appearance in ontogeny relatively to black, convince the author that red in itself, or in the form of orange or yellow, is the most primitive colour in the Pieridae; that black and the dark tints are more specialised, and that as they develop the original red diminishes in amount and fades to orange, yellow, and ultimately white. The process which results in this gradual decoloration of the primitive pigment does not, however, end at white, but ultimately results in the loss of the scales themselves; forms like *Macroglossa fuciformis* L., and *M. bombyliformis* Ochs., with their glassy wings, being regarded by the author as terminating the course of evolution along this line. Simultaneously with the decoloration of the primitive red pigment, there may be a development of structural colour, but the relation of this to the dark pigments

is unfortunately not considered. The obvious difficulty that in ontogeny white appears before yellow or red, the author ingeniously removes by pointing out that this early white is probably optical, while the white colour of the adult is due to uric acid acting as a white pigment.

We cannot here follow our author into his discussion of the bearing of his results on the chief controversies in regard to colour phenomena, but their relation to concrete details may be noticed.

In the first place, the view that bright scarlet is the most primitive colour in the Pieridae, with the correlated statement that the males in the family are frequently more primitive in regard to their colours than the females, will be found somewhat difficult of acceptance by most. At the same time, when it is recollected that the examples chosen were largely tropical forms, that the red and yellow pigments are chemically nearly related, and that there is some evidence to show that various external conditions, especially heat, are of importance in the production of red varieties from forms normally yellow, it would seem that our author's statements can be reduced to the simpler form that red, yellow, and white are primitive colours in the Pieridae, and, in nature as in the laboratory, are readily converted into one another. When we remember that these three colours in the Pieridae are due to uric acid and its derivatives, there seems nothing improbable in the suggestion that in the course of evolution they tend to disappear as the dark pigments develop, and are replaced by the brilliant optical colours which are apparently associated both with these dark pigments and with the progressive differentiation of the scales. Such a suggestion I have indeed already made in "Colour in Nature." Piepers seems, however, disposed to apply his theory not only to the Pieridae but to butterflies in general, in which pigments certainly allied to uric acid have not yet been demonstrated. We have just seen also that Baer's work tends rather to emphasise than to destroy the distinctions between the pigments of the Pieridae and of other butterflies. On the other hand, the fact that in the case of species of *Papilio* and *Vanessa* the Countess Maria von Linden found that black was the last colour to develop in ontogeny; that the wings of pupae were first yellowish-red; and that those early colours were later concealed by the development of dark scales on the top of the light ones, certainly suggests a course of evolution similar to that occurring in the Pieridae. The question is not likely to be decided until we know the exact relation of the bright pigments of other butterflies to those of the Pieridae. Nor can we hope to clearly understand the colour phenomena of the Lepidoptera in general, until we also know the relation between the dark and the bright pigments,

the origin of both, and the relation between the development of dark pigments and of optical colours. When these points have been determined, we may hope to be able to take a fresh survey of the whole field of colour, with some prospect of reaching definite conclusions as to the immediate cause of colour variation. The colour variations of the Lepidoptera have always been objects of great interest, and it may be looked upon as a hopeful sign that so many biologists are endeavouring to discover their evolution value, instead of being content to point out how they may be useful to the species.

Finally, whatever may seem doubtful or speculative in Piepers' suggestions, his observations, together with those of the other authors named, point at least to one definite conclusion which is worth emphasis. They seem to confirm the view, supported by many recent investigators, that there is an intimate connection between the colours of organs and their general structural peculiarities. Thus, in the present case the colours of individual scales do not change very markedly in development, but the early scales with their primitive tints are covered up by scales bearing the hues of the imago. Similarly, in the adult, scales exhibiting the more primitive colours are of simple structure, and are replaced in phylogeny by scales of more complex structure and more specialised tint. This is a point of much importance, for it tends to show how far colour changes are from being superficial, how intimately they are connected with the other characteristics of the organism.

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# The Red and Blue Colouring Matters of Flowers.

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In my previous paper (see *Natural Science*, 1898, xii. pp. 194-199) it was maintained that the tannic chromogen of the blue colouring matter of flowers is not the same as that of the red pigment. The old investigators of this attractive subject were not all of one mind in the matter. The great majority unequivocally held that the blue was the original colour, and that the red was merely a derivative thereof, *i.e.* merely an unessential modification, or a product of further deoxidation or dehydration. The French chemist Filhol was for a time evidently in a state of considerable doubt and hesitancy. In his last contribution on the subject, dated 25th June 1860, he states that in examining the colouring matter of red, rose, or blue flowers he had been struck with the differences which it presents according as it is taken from such or such a flower; he noticed that a great number of flowers become blue in contact with alkalis, while others become green, and, moreover, that the pigment of the former enjoys a greater stability than that of the others. "At first," he says, "he thought he could conclude that in deep red flowers there exist two distinct colouring matters, one more stable than the other, but he soon saw that it was not so when he operated on solutions of pure cyanine; there are not two kinds of cyanine." It is all the more remarkable, therefore, that the illustrious Berzelius, who had chemically examined the red pigments of cherry and gooseberry fruits and leaves, should conclude that their colouring matter is not, as had been thought, a combination of a blue pigment with an acid. "What had given occasion for this error," he says," "is that by treating the juice of the berries with acetate of lead blue precipitates are obtained, but this blue colour is due to the impurity of the juice in consequence of the presence of citric and malic acids." It is hardly necessary to observe that for a very long time it was well known to chemists that the blue colours of flowers were reddened by acids, and even that the tint of flowers naturally red was made more vivid and brilliant by the addition of a trace of acid. But this fact, remarkable as it is in its way, seemed comparatively trifling so long as the various and divers pigments of plants were regarded as

merely modifications of only one fundamental substance. Tainted with German alchemical mysticism, Schubler and Frank, A. P. De Candolle, and Marquart successively sought for and insisted on the close connection and genetic relationship of the reds, blues, and yellows: they went further, they proclaimed that the colours of flowers were all derived from and indissolubly bound to chlorophyll.

Now, it is obvious that no thorough and complete scientific understanding of this subject could possibly be attained so long as this sort of poetically "evolutional" imagination held the field. How eminently, then, does it stand to the credit of those acute and able chemists, Frēmy and Cloez, Martens and Filhol, that they were not moved or deterred in the slightest degree by the *à priori* postulates of their German predecessors, that, in fact, they entirely ignored the connection of these brilliant colours with chlorophyll. No doubt Filhol declared that "there is the most direct analogy between xanthine and chlorophyll," but he never sought to derive the one from the other. He and his collaborateurs examined the floral pigments in and by themselves, they were never troubled with evolutionary imaginations and hypotheses, and hence they led the way to discoveries which fitly served as bases and props for further research and enlightenment. Berzelius had little doubt that pure chlorophyll may be the origin of the yellow and red colouring matters of autumn leaves, but he admits that he was never able to reproduce chlorophyll by means of xanthophyll, nor transform chlorophyll into xanthophyll. Mohl, in 1837, would not admit that chlorophyll has any relation with the red colour of autumn leaves, but he does not deny its intervention, though only indirectly, in the red colour of fruits; there is nothing to show, however, that he considered that there was any chemical connection between these bodies. In 1850 Morot, in criticising Marquart's views, observes that "chlorophyll is not found in the most superficial layers of cells where the blue, violet, and red colouring principles are chiefly found; in the more deeply situated cells of the mesophyll there exists much chlorophyll, and at a certain epoch the red substance is seen to take birth there, but at the same time the chlorophyll persists, and this red substance seems to proceed from cell sap, which is colourless beforehand; this sap by the prolonged action of a weak acid becomes red without passing by blue." This passage seem to me to be the very first distinct and definite declaration on chemical grounds, partly at least, that the green matters of leaves are not connected with the red and blue matters, although, as Morot admits, it may be affirmed that chlorophyll, anthocyan, and the red matter may be modifications of one and the same substance; but it is hardly necessary to rejoin that this is totally different from the affirmation that the blue pigment is a direct derivative by dehydration of chlorophyll itself, as Marquart maintained.

In 1858 Morren published his famous "Dissertation sur les feuilles

vertes et colorées," in which he, with great acuteness, insisted that the red and blue colouring matters of plants are not formed from chlorophyll; and the blue colouring matter (anthocyan) is probably, like litmus, the alkali salt of an acid which in the free state constitutes the soluble red pigment (erythrophyll) of autumn leaves. It will be noticed also that Morot had clearly enounced that a red colour could possibly be formed independently in the leaf, in fact it is seen to be produced without having previously passed through a blue tint; in other words, it was not derived from the blue. Here, then, there was a further break up of the olden entanglement, for not only were the blue and red separated from the green, but the red was threatened with divorce from the blue. Now, it was just precisely at this critical point in the scientific enlightenment of the mystery that two papers were published which may rank among the more interesting efforts of an epoch fertile in important researches and discoveries.

In the *Botanische Zeitung*, dated April 18, 1862, there appeared an article entitled "Some Propositions anent the Physiological Meaning of Tannin and of the Pigments of Plants" by A. Wigand, in which the author, after giving various illustrations, and leaving chlorophyll, anthoxanthin, indigo, etc., out of account, avers that "in general it results from the foregoing as to nearly all blue and red pigments that these proceed from tannin through an only unessential modification." . . . 'A colourless substance dissolved in the cell sap furnishes the basis for anthocyan, and this chromogen is tannin, or rather some modification of tannin (cyaneogen): the transformation of cyaneogen into anthocyan depends on an oxidation." Again, early in 1863 Professor W. Stein suggested that the red colouring matter of flowers was paracarthamin, a red substance which he had obtained by the action of sodium amalgam on the plant-yellow (rutin) that had been discovered by Weiss in 1842.

Notwithstanding the extreme suggestiveness of the declarations just now set forth, appealing as they did most forcibly to the scientific intelligence, it is extremely discreditable that, so far as concerns Germany, they were practically ignored for nearly twenty years. No doubt Wiesner, alluding to Wigand's paper, had published some observations about eight months afterwards, and again in 1872; and Kraus, in 1872, had connected the red coloration of winter leaves with the presence of tannin. But it was not till 1881 that Detmer confirmed the views of Kraus, and H. Pick, in 1883, by an elaborate histological and micro-chemical investigation of young shoots, older stems, petioles, fruits and their peduncles, and autumn leaves, fully and amply extended and ratified the propositions enunciated with such brilliant genius by A. Wigand. "We now," he states, "on the basis of our researches, assert with Wigand, as a fact, that the red pigment is to be found only in plants containing tannin, and that the tannin, at first colourless, can change into the red pigment, which likewise reacts like tannin."

Meanwhile, however, the remarkable differences which are observable in red and blue flowers and their colouring principles towards certain reagents, especially dilute ammonia, or ammonia vapours with or without the presence of added acids, still continued to excite comment and acute controversy. So far back as the year 1824 Macaire of Geneva, having noticed that a red infusion of *Viola odorata*, when mixed with a vegetable alkaloid, such as quinine or strychnine, gradually retakes the natural blue tint of this flower, suspected that its colour is owing to a combination of its chromule with an alkali. In 1825 Schubler and Frank testified that the infusion of *Funkia ovata* treated with an acid and then with an alkali may present in the same vessel all the colours of the spectrum. De Candolle was perfectly aware, in 1832, that the infusion of certain red flowers which have been acidified assume a blue colour when treated with alkalis, and, on the other hand, that the acidified infusions of blue flowers do not reassume their original colour when similarly treated. Frèmy and Cloez asserted that anthocyan was reddened by acids and greened by alkalis. Wigand stated that the colouring matter dissolved in acid cell saps became by alkalis first blue and then green. Wiesner, on the other hand, stoutly maintained that anthocyan as such or in itself was always blued by alkalis and never greened: it is only when it is present along with a substance which is coloured yellow by alkalis that it passes by the latter body into green, which thus arises as a mixed coloration. In 1867 Naegeli and Schwendener again asserted that anthocyan is blued by alkalis and then passes into green; and Sacchse, in 1887, expressed concurrence with this view. All this diversity of opinion concerning a matter which, one might imagine, is capable of being settled once and for all by the application of definite tests, would undoubtedly never have come to pass if there had not been "something in it," as they say. The remainder of this paper will be devoted to an attempt to explain precisely what this mysterious "something" is.

In the first place, it will be advisable to enumerate at least some of those flowers whose colouring matters are known to be blued by ammonia vapour and by a dilute solution of ammonia. The following list comprises some of the most notable examples:—*Fuchsia*, *Pelargonium* sp., *Plumbago*, blue sp., *Lycium barbarum*, *Phascolus multiflorus*, *Erythrina crista-galli*, *Echinacea scrotina*, *Impatiens balsamina*, *Salvia splendens*, *Polygonum orientale*, *Camellia*, Paeony, and deep-red garden Rose (acidified alcoholic extracts). There are doubtless many more, but the study of these may suffice to throw some light on the subject. Assuming what has been abundantly proved, viz., that tannin constitutes the chromogen of the red and blue floral pigments, and that tannin is not a homogeneous chemical compound, but embraces many varieties, we must ascertain if there exists any kind of tannin in the aforesaid flowers different from that which occurs in the

ordinary red or blue flowers which are coloured green by dilute alkalis. Now, it would appear that the majority of flowers contain either rutin or an iron-greening tannin, but a select few, including *Fuchsia*, *Pelargonium*, *Paeonia*, and *Camellia*, undoubtedly contain an iron-blueing tannin which is no other than gallo-tannin mixed probably in some older specimens with a small quantity of gallic or ellagic acid. The ordinary pink rose contains only a highly phloroglucin-bearing tannin common to the whole order to which the queen of flowers belongs; but in certain garden varieties, wherein the petals have immensely developed with full expansion of parts and profound depth of coloration, a small proportion of gallic acid has doubtless managed to find entrance into the cells of the corolla. In fact, Filhol, Rochleder, and others found gallic acid as a constituent in the well-preserved and intensely purplish crimson cones of the officinal *flores Rosae rubrae;* and similar remarks will apply to the case of *Polygonum orientale.*

The residue of the flowers in the foregoing list belong, it will be observed, to the orders Solanaceae, Plumbaginaceae, Labiatae, Compositae, and Leguminosae, all of which are distinguished by the absence of free phloroglucin, a fact which affords an insurmountable presumption that the tannic chromogen is of a character distinct from that with which we have just dealt. All who have had some experience in the chemistry of herbaceous plants are fully aware that they are brimful of quercetin in the form of rutin, etc., which, however, of itself fails as a basis for brilliant colorific effects, but may, nevertheless, by further de-assimilation develop into tannins which, in the special instances now under review, belong to the distinctively iron-greening class. The question which is now started and remains to be discussed is, whether this iron-greening tannin is adequate to discharge the function of a chromogen competent to evolve a pure blue flower?

All the genera mentioned are, with the exception of *Erythrina*, capable of producing a blue or purple flower in some of their specific forms. *Fuchsia*, *Plumbago*, *Lycium*, and *Salvia* produce one or more pure blue efflorescences; while *Pelargonium*, *Phaseolus*, *Echinacea*, *Impatiens*, *Polygonum*, *Camellia*, *Paeonia*, and *Rosa* produce purples more or less deep and frequently approaching deep blue. Of all these, *Fuchsia*, *Plumbago*, *Pelargonium*, *Lycium*, *Polygonum*, *Camellia*, *Paeonia*, and *Rosa* contain either an iron-blueing tannin or gallic acid in small quantity, and it is the colouring matter of just these flowers which is most distinctively blued by ammonia vapour or solution. In point of fact, I think it must needs be concluded that in all these instances it is the gallic acid resulting from the oxidation of gallo-tannin or of some nearly allied benzene derivative, which is solely responsible for the blue more or less pure and clear which they so beautifully display. *Rosa* and *Polygonum* are exceptional, inasmuch as they are genera which contain a highly phlobaphenic tannin, *i.e.* a chromogen which

on advanced oxidation evolves brown-red or muddy anhydrides more than sufficient to neutralise and overcome any tendency to blue coloration incident to the presence of gallic acid.

Nevertheless, it is evident that gallic acid is not the only substance that may officiate as chromogen in the outcoming of vivid and brilliant blues. The genus *Linum* presents a remarkable phenomenon in this connection. Of some fifty species of this genus about seventeen are pure blue, and the rest are either yellow or purple, lilac, crimson, rose, pink, or pure white. The most astonishing species is *L. grandiflorum* (*coccineum*) which is the one crimson amidst a genus predominantly blue: it contains no gallotannin or gallic acid, but there is some rutin and iron-greening tannin, and there is a small amount of free phloroglucin in the plant itself. Now, if ever there was a red flower which is or has been naturally and originally blue it is this one, as the following reactions will show. The alcoholic extract of the petals is of a magenta-red colour, and the filtered aqueous extract of its evaporated residue yields with acetate of lead a blue colour, and then when acetic acid is added in large excess the *blue* colour still remains; with subacetate of lead it gives a pure blue colour; with bicarbonate of soda a grayish-blue turning grayish-green, and with oxalic acid added a bright scarlet red is obtained; with acetate of magnesium a splendid green, and when acid is added the original red is restored. Thus we see that in the presence of heavy bases the blue coloration persistently remains even in the presence of a considerable excess of free organic acid; with light and feeble bases, on the other hand, the addition of a small quantity of acid determines a distinctly red reaction. And all these facts seems to me to demonstrate what I consider to be quite exceptional, viz., that in *Linum coccineum* the red coloration of the flowers is produced by the accidental presence of free acid affecting a naturally blue pigment. It is hardly necessary to subjoin, that in the vast majority of cases, whether the flower be either red or blue, the particular tint is not influenced or created by the neutral or acid condition of the cell sap.

By what circumstances, then, is the particular tint created or influenced? In order to answer the question satisfactorily two facts must be minutely considered:—1. Chemical, the presence of quercetin in the form of rutin, etc., in the corolla. 2. Physiological, the possession by the corolla of energetic respiratory and transpiratory functions, with the result that the substances contained in its cells undergo an oxidation more or less vigorous and complete. In my previous paper I showed that the red pigments of flowers were specifically incidental to tannic chromogens which contain phloroglucin in their molecules and yield phlobaphenes, *i.e.* a series of anhydrides, the lower of which are crimson and the higher are red-brown; while the blue pigments were incidental to acid tannins or to tannins which yield acids on oxidation. Now, only a slight chemical experience is requisite for the under-

standing that in the latter case the process of oxidation has been more energetic and more complete than in the former case. Even where the blue is apparently the result of a combination of a tannic acid with a base, the effect cannot be produced save under circumstances especially favourable for oxidation. The flowers comprised within the great division Corolliflorae having a gamopetalous corolla, enjoy, by reason of the great expansion of cellular surface, a respiratory and transpiratory activity which the Polypetalae cannot exhibit. We see that all or most of our decidedly blue flowers are gamopetalous, *e.g.* gentians, bell flowers, Jacob's ladder, convolvulus, speedwells, various labiates, borage, etc. *Linum perenne* is an example of a bright cobalt blue-coloured polypetalous flower; but it may be readily observed that the petals are very broad and generally large in comparison with the thin and wiry stem and the small strap-shaped leaves; and I know of no valid reason why, under such propitious circumstances, the quercetin of the cell sap may not in the absence as here of any phlobaphenic tannin, develop to the full, and sowise evolve a high oxybenzoic acid which, being immediately transformed into pigment, may not be readily detectable in the free state.

In view of the researches already published, and of my own experiments, I think it may be concluded, that—1. A blue flower is unproducible in species which contain or are capable of forming phlobaphenic tannin, no matter what the development of the inflorescence may amount to. 2. A blue flower is more likely to be produced in a species having a gamopetalous corolla or perianth, and therefore liable to evolve by higher oxidation a certain quantity of a high oxybenzoic acid. 3. In species wherein the tannin natural to the organism is iron-greening and non-phlobaphenic, a blue flower may possibly be producible in a polypetalous corolla, provided always that the petals or perianth be large relatively to the height of the plant and to the size and robustness of its stem and leaves: in this case it is uncertain whether gallic acid is necessary for the production of the effect, but anyway an alkaline compound of an oxybenzoic acid would seem to be indispensable. I hope in a concluding paper to specifically examine these three propositions, and to supply full illustration of their scope and tenor.

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# FRESH FACTS.

Nineteen Days without a Head. Charles Janet.—"Réaction alcaline des chambres et galeries des nids de Fourmis. Durée de la vie des Fourmis décapitées" (*Comptes Rendus Ac. Sci. Paris*, 1898, cxxvii. pp. 130-133). Janet had decapitated some workers of *Formica rufa* in his search for the little Nematode worms (*Rhabditis*), which are parasitic in the head, and was struck by the vitality of the headless bodies. In a moist chamber they all remained alive for several days, and moved their limbs when gently stimulated. Three lived for two days, one for three, two for five, one for seven, two for nine, and one for nineteen. The last was a large worker, with an abdomen distended with food.

Uses of the Swim-Bladder. Chr. Jacobs.—"Ueber die Schwimmblase der Fische" (*Tübingen Zool. Arb.*, 1898, iii. pp. 385-411). The swim-bladder of the eel contains 44·74 per cent of oxygen, 51·97 per cent of nitrogen, 3·29 per cent of carbon dioxide, and this mixture is reduced in amount when the eel is kept out of water. The gas is *probably* liberated by the vascular wall of the air-bladder, and the oxygen is *probably* absorbed again by the vascular wall of the pneumatic duct. In pike and perch there seems to be no respiratory function, but the organ *probably* serves to regulate the specific gravity of the fish. In *Cobitis fossilis*, which can live in non-aerated water by gulping air at the surface, the small, apparently rudimentary, swim-bladder is enclosed in a bony capsule, and can hardly be supposed to help in breathing. This function seems to be discharged by the thick capillary network beneath the epithelium of the intestine.

Witch's Milk. S. G. Tschassownikow.—"Chemische Untersuchung der Hexenmilch" (*Le Physiologiste Russe*, 1898, i. pp. 68-72). The milk which is formed by the mammary glands of both sexes soon after birth, attaining its maximum in the third week, and disappearing completely during the next fortnight, has often been examined microscopically, and is well known to contain the same corpuscles as ordinary milk, but it has been analysed only three or four times. The last analysis shows that the component substances are the same as in the milk of normal lactation, but the proportions are different. It contains more water and salts, but much less proteid, sugar, and fat. Its proportions closely resemble those of asses' milk.

Making of Muscle. Alex. Meek.—"Preliminary note on the post-embryonal history of striped muscle fibre in Mammals" (*Anat. Anzeig.*, 1898, xiv. pp. 619-621). In the field-mouse, the cat, and the tame rat, hypertrophy of the fibres occur, accompanied by a reduction in their number,—a good instance of "the struggle of parts within the organism," as Roux phrased it.

What is a Brain? J. Steiner.—"Die Funktionen des Centralnervensystems und ihre Phylogenese," iii. Abth. Die wirbellosen Tiere (Braunschweig, 1898; see *Biol. Centralbl.*, 1898, xviii. pp. 749-751). The author

has defined a brain as the general motor centre associated with at least one of the higher sensory nerves. In this sense there is a brain in the supra-oesophageal ganglia of Crustacea and Tracheata, but not in Molluscs, Annelids, Nemertines, or Planarians, not to speak of Echinoderma and Coelentera. In the dorsal ganglion of *Octopus* there seems to be a cerebrum in the Vertebrate sense, but not a real "brain."

EVOLUTION OF THE BRAIN. B. HALLER.—"Vom Bau des Wirbelthiergehirns," I. Theil, Salmo und Scyllium (*Morphol. Jahrb.*, 1898, xxvi. pp. 345-641, 11 pls. and 23 figs.) As a speculative conclusion based on a multitude of facts, Haller suggests that the ancestor of the Chordata (the Helminth, to wit) had for its central nervous system a pair of supra-oesophageal ganglia and lateral nerve cords. The latter approached dorsally and coalesced in a single cord, whose central canal remains as a sign of the primitive separateness. The canal was continued forward to the primitive brain, probably the region of the ganglia habenulae, where a similar cavity arose by a fusion of the ventral parts of the ganglia. Thereafter followed a metameric differentiation of the nerves, both cranial and spinal, while the formation of the Vertebrate eyes and the concentration of the olfactory spheres gave an early impulse to anterior cerebral differentiation.

CHANGES IN THE DENDRITES. S. SOUKHANOFF.—"Contribution à l'étude des modifications que subissent les prolongements dendritiques des cellules nerveuses sous l'influence des narcotiques" (*La Cellule*, 1898, xiv. pp. 387-395 1 fig.) "L'anatomie pathologique de la cellule nerveuse en rapport avec l'atrophie variqueuse des dendrites de l'écorce cérébrale" (*T. C.*, pp. 399-415, 4 figs.) In the guinea-pig intoxicated with trional, poisoned with arsenic, subjected to thyroidectomy, or otherwise treated so as to bring about marked nutritive disturbances, the dendritic prolongations in the cells of the cerebral cortex become markedly moniliform or varicose, which probably implies a degenerative process associated with relative atrophy. The particular interest of this is, that traces of the moniliform state are to be seen in the normal condition.

THE LENS OF THE MOLE'S EYE. C. RITTER.—"Die Linse des Maulwurfes" (*Arch. f. mikr. Anat.*, 1898, liii. pp. 397-403, 3 figs.). The minute lens is without a nucleus and without concentric structure, in fact, rather like a half-made lens in the frog. It permits the passage of light, but is quite incapable of forming an inverted image. If any image is formed on the retina it can be little better than a tangle of lines.

A SHOWER OF FOSSILS. LORTET.—"Chute de Crustacés ostracodes fossiles observée à Oullins, près de Lyon, le 24 Septembre 1898" (*Comptes Rendus Ac. Sci. Paris*, 1898, cxxvii. pp. 1231, 1232). On a calm evening, about sunset, Lortet observed, near Lyon, a thick and rapid shower of fossil Ostracods (*Cypridinia*), and heard the minute shells rustle the withered leaves. They had no doubt been caught in an ascending air-current from some of the regions in North Africa, where they abound.

# SOME NEW BOOKS.

## HERTWIG'S THEORY OF BIOGENESIS.

Die Zelle und die Gewebe. Grundzüge der allgemeinen Anatomie und Physiologie der Gewebe. Zweites Buch. By Prof. Dr. Oscar Hertwig. 8vo, pp. viii. + 314, with 89 illustrations. Jena: Gustav Fischer, 1898. Price 7 marks.

Although this book comes at an opportune moment, when the controversial fires of the "evolutionists" and "epigenetics" are burning low, and contains many interesting observations, it will not, in our opinion, mark any great epoch in the history of the subject. Even the most determined opponent of the neo-evolutionistic school will probably admit that Weismann's latest most important work, "The Germ-plasm," is, in some respects at least, a work of genius; but this could be hardly said of the present volume. Indeed, after reading the three hundred odd pages it contains, we must confess to a feeling of disappointment that Dr. Hertwig has not been able to state a stronger case for himself and his colleagues. As a destructive critic he is generally convincing when he attacks the "evolutionists," and he has no doubt done valuable service in overthrowing much of what was at one time freely accepted by many biologists as fact, which can now be no longer upheld in the light of recent observation and experiment. But we much doubt whether his "Theory of Biogenesis," which explains development partly from an "evolutionistic" and partly from an "epigenetic" standpoint, will be very widely received. We believe that many of Dr. Hertwig's deductions are founded on unsound premises. For instance, we hesitate to place very much value on the results of those experimental embryologists, *e.g.*, Loeb and Herbst, who submit developing organisms to conditions seemingly impossible in a natural state. Their results are interesting, and deserve to be carefully noted, but they can very easily be made to prove too much, and then become exceedingly dangerous pitfalls from which, in our opinion, Dr. Hertwig has not escaped. He is, however, much to be congratulated in fighting shy of metaphysics and reserving himself to the facts of embryology, cytology, and medicine and surgery, taken from sources probably unknown to the general biologist; while there can be no doubt that a careful study of the work will open out many new paths of research. Herein, we think, lies the great merit of the book.

The first four chapters are devoted to a comprehensive study of the cell in relation to its neighbours, and deals especially with the organic connections between the cells composing the bodies of animals and plants. The author then goes on to point out how many external forces there are continually at work in the environment of an organism which produce the most complicated effects on it, but these depend also on the nature of the organism itself. The fact that fertilised ova of two very different forms develop in what are apparently identical circumstances, shows that much depends on the internal molecular structure of the ovum. This leads on to a comparison of the rival theories of the "evolutionists" (Weismann,

Roux, Poulton, etc.), and of the "epigenetics" (Hertwig, Driesch, Morgan, etc.), in which he satisfactorily disproves Weismann's fundamental assumption of the "qualitative" (*erbungleich*) division of the idioplasm of the nucleus, but yet admits that such an idioplasm must exist, and so paves the way towards an explanation of his "Theory of Biogenesis." Development, according to Hertwig, depends on two sets of factors: (*a*) external; (*b*) internal. Under the first he groups the various effects of environment, and discusses at length the influence of such forces as gravitation, stress, and pressure in the formation of organic tissues. Much space also is devoted to the influence of light, temperature, and various chemical substances on developing animals and plants. Among the internal factors Dr. Hertwig deals chiefly with the correlation existing between the cells themselves and between the organs they compose. These chapters form perhaps the most important part of the book, for although it is generally admitted that correlation must play an enormous part in ontogeny and phylogeny, yet in text-books it is rare to find more than a few trite instances quoted, such as the deafness of blue-eyed white cats, etc.

We have also a mass of information with regard to the latest results of experimental embryologists as to the phenomena of regeneration. After discussing the many pathological changes undergone by cells, Dr. Hertwig finally deals with such controversial points as the inheritance of acquired characteristics, and the part played by the idioplasm of the cell itself, and concludes with a sketch of the way in which his theory has gradually been evolved. It may be briefly summarised as follows:—

(1) Every cell has its own specific characteristics.

(2) The hereditary material which one cell passes on to another is lodged in the nucleus.

(3) The invisible specific distinctions of an ovum give rise to the visible specific distinctions of an adult organism solely by multiplication of cells, and their actions and reactions upon one another.

(4) The differences of cells are not produced by their receiving different portions of the hereditary substance. All receive like shares, but do not respond alike to external and internal stimuli. The fate of a cell largely depends on the exact position it holds with regard to its neighbours.

(5) Except in the lowest forms of life, each cell, as development proceeds, gradually loses the power of reproducing the whole organism, but the germ cells are exceptions to this rule.

(6) Besides possessing hereditary substance, a cell, if it is to develop into an organism, must possess the power of responding to certain external conditions. Unless these latter are present development is impossible.

In reviewing this book, all that has been possible to do is to give a bare outline of Dr. Hertwig's methods and general aim, and we must leave the reader to draw his own conclusions. Many, no doubt, will be satisfied with the results arrived at. All will congratulate Dr. Hertwig on the completion of his work, albeit some may be more than ever convinced that the end of this century sees us still in almost complete ignorance of the causes of organic development.

M. D. HILL.

## LOST LINKS SUPPLIED.

The Last Link. Our Present Knowledge of the Descent of Man. By ERNST HAECKEL. With Notes and Biographical Sketches by HANS GADOW. 8vo, pp. 156. London: Adam and Charles Black, 1898. Price 3s. 6d.

About half of this little book, which seems dear for its size, is occupied with the address which Professor Haeckel gave at the Congress of Zoologists at

Cambridge, the other half contains Dr. Gadow's notes, and it is difficult to say which half is the more interesting. With his wonted clearness and decisiveness Haeckel sums up the conclusions set forth at length in his "Systematische Phylogenie," and traces the presumed evolutionary stages from Monera to man. Due use is made of *Palaeospondylus* and *Pithecanthropus* and other recent discoveries, while some hypothetical groups are utilised to eke out the imperfect geological record. The essay must be taken as a sketch of phylogenetic possibilities—and the possible has a wide radius in phylogeny—for it need hardly be said that there is room for much difference of opinion as to the systematic position of many of the types. What we are afraid of is that the uncritical and unsuspicious may infer from this lucid exposition that there is no longer any doubt that "the Dipnoi form the actual link between fishes and Amphibia," that "*Sphenodon* is the reptile which stands nearest to the main stem of our ancestry," or that the acknowledgment of the affinity of Vertebrates and Tunicates "has abolished the erroneous hypothesis that the Vertebrata may have arisen from Annelids or from other Articulata." These and many similarly decisive statements may turn out to be true, but surely there is at present warrant for a reasonable argument over any of them. We have great admiration for Haeckel's genius, but his decisiveness often becomes perilously like dogmatism; nor can we conceal our disappointment to find him saying that it would be better to return to the Mosaic cosmogony than to agree with Weismann.

Dr. Gadow furnishes terse and vivid biographical sketches of Lamarck, Etienne Geoffroy Saint-Hilaire (variously referred to also as Geoffroy, Geoffroy Saint-Hilaire, and Saint-Hilaire), Cuvier, von Baer, Johannes Mueller, Virchow, Cope, von Koelliker, Gegenbaur, and Haeckel himself. Besides these sketches, there are notes on the theory of cells, factors of evolution, and geological time. Here and there we notice what appear to us to be defects in the historical notes, and we venture to give three illustrations. In discussing Lamarck's position is it not well to point out that, as regards the evolution of plants, Lamarck laid the main emphasis on the direct action of the environment? In speaking of Etienne Geoffroy Saint-Hilaire, Dr. Gadow says "he maintained that, since Nature takes no sudden leaps," etc., but is it not to this author that we owe some of the first suggestions as to saltatory or transilient variations? Dr. Gadow credits Huxley with the terms "ectoderm" and "endoderm," but were they not Allman's invention? We might ask other such questions, but it seems ungracious, for the notes are delightful. Not least so is the one with which we disagree most heartily—that which maintains that adaptation and variation are simultaneous and fundamentally the same, and that acquired characters are inherited. X.

## GEOMORPHOLOGY.

Earth Sculpture. By PROFESSOR GEIKIE, LL.D., D.C.L., F.R.S. 8vo, pp. xvi. + 320, with 2 plates and 89 figures. Progressive Science Series. London: John Murray, 1898. Price 6s.

During the Victorian era the origin of the earth's surface-features has been studied by many geologists, especially by those whose vocations have kept them much out of doors. Ramsay, Jukes, Whitaker, Foster and Topley, Lapworth, both the Geikies, and others in Britain, and a host of workers in America and on the Continent, have all contributed to our present knowledge of the genesis of land forms. During the last ten or fifteen years many of those whose business it had hitherto been to record geographical facts without concerning themselves as to how the phenomena had been brought about, have awakened to the importance of learning *why* these features have taken on their present form. In

other words, geographers are now realising that if we are to obtain any scientific knowledge of geographical forms, we must first discover how these forms have arisen. Geologists have for many years concerned themselves very much regarding this particular question, and now their fellow-workers the geographers are realising the importance of making up for lost time, and are specialising more and more every year in their studies of the origin of land features. So rapid are the strides that have been made in this direction within the last decade that the geologist is already beginning to be left behind. Those who have closely followed the development of the science of Geomorphology are fully aware that its foremost exponents at the present day are to be found in America; and on the Continent there are arising many workers who bid fair ere long to distance even their American brethren.

Thus it happens that the development of Geomorphology has reached a phase at which that science forms a kind of debateable land between the respective territories of the geographer and the geologist; and, indeed, it seems destined in the near future to take up a position as an independent science, which will bear much the same relation to its parent sciences that Mineralogy does to Chemistry and Mathematics, or that Palaeontology does to Biology and Geology.

For many years Professor Geikie has been one of our foremost exponents of this branch of his science, and in the handsome volume of nearly three hundred pages which Mr. Murray has published under the title of "Earth Sculpture," the author has given us a summary of his own views on the origin of surface-features in general, and in particular of those of his native land. The book is written with that lucidity of style for which the author and his brother have so long been famous. The get up of the book, as one might have expected from the reputation of the publishing firm, is in every way excellent. Besides the 89 figures in the text, there are two reproductions of photographs by Mr. W. E. Carnegie Dickson, showing in an admirable manner the mural weathering of granite in Glen Eunach, in the Cairngorm Mountains. J. G. G.

## HENRY DRUMMOND.

The Life of Henry Drummond. By GEORGE ADAM SMITH. 8vo, pp. 506, with portrait. London: Hodder and Stoughton, 1898. Price 7s. 6d.

The author, who was intimate with Drummond from their college days until the end, has gathered all possible material for his work, and no one else could have written the book half so well. Its only fault was inevitable; one learns of the extent of Drummond's life, but its peculiar harmony is somewhat lost; memory is still too particular and sharp; the book is new wine.

Perhaps the author might have estimated more highly the significance of Drummond's larger writings. They are the only spontaneous and ingenuous flower of the doctrine of design within many years. Drummond lived in the impression of nature and experience as one and divine; it was accidental that as Böhme, under the same impulse, used astrological terms, so Drummond spoke in the no less inadequate language of current scientific doctrine. He found the unity of the world neither in its dust, nor in a blind monstrous will, nor in a dead system of concepts, nor in anything poorer than man, but in that which is at least human all through. He seriously attempted a Theistic and even a Christian doctrine of nature and experience, and he studied living creatures and men's lives in detail to that end. His work was premature; it was an impression rather than a construction; but it stands as an isolated example of that attempt without which theology cannot live. But the student was only part of the man, and probably not the chief part; these pages show an experience and a nature of unusual width and graciousness. S.

## VERTEBRATE PALAEONTOLOGY.

Outlines of Vertebrate Palaeontology for Students of Zoology. By ARTHUR SMITH WOODWARD. 8vo, pp. xxvi + 470. Cambridge: University Press, 1898. Price 14s.

This is a work to which all students of vertebrate palaeontology have been looking forward with lively interest, both because of the reputation of its author, and of the peculiar advantages which the great collections of the British Museum offer for fresh and original treatment of the subject. The volume is to be read and reviewed for what it purports to be, as explained by the author in the preface and elsewhere, namely, a brief sketch of the palaeontology of the Vertebrata, intended for students of vertebrate morphology and zoology, who are desirous of examining in detail the palaeontological aspect of their subject. It need not be placed beside works of a monographic character, nor even more exhaustive educational works, such as Zittel's "Palaeontologie." And it is greatly needed for the very purpose which the editors had in mind, namely, for students of the various branches of vertebrate zoology, for it is becoming more clear daily that the educational gap between living and dead vertebrates is entirely unnatural; that there are no vertebrates which can be thoroughly understood without recourse to palaeontology. A notable instance of the danger of ignoring palaeontological evidence is found in the numerous errors which persist in the otherwise magnificent "Vergleichende Anatomie" of Gegenbaur, recently published as the consummation of a lifelong work.

In most respects the "Vertebrate Palaeontology" meets its purpose admirably, and can be placed in the hands of students with the teacher acting as a guide. In other respects it is somewhat open to criticism when compared with the best models of didactic writing. Moreover, the prevailing knowledge of Palaeontology is so limited, that the majority of teachers will not be able to give the critical suggestions and corrections which are here and there necessary, in order that the treatment may be transparently clear. This is due, perhaps, to the fact that the author, as an eminent specialist who has not given particular attention to teaching, overestimates the capacity of his student readers. It is true that the style is lucid and admirable, the drawings are well selected and beautifully executed—a very large number of them being original—but in many cases there is an absence of the concise definition and sharp demarcation of the characters of different groups which one finds in such a model work as Huxley's "Comparative Anatomy of the Vertebrates."

While the subject is logically developed, each section being introduced by a concise description of some typical form, there is a looseness and uncertainty in the use of scientific terms and nomenclature which would leave the average student in a state of confusion. For example, the cartilaginous supports spoken of as "endoskeletal rods" (page 19), are defined as "baseosts" (p. 20), where they lie within the fin proper, and the same elements are termed "radial cartilages" later on (p. 30). Similarly, the rods lying within the body are defined as "axonosts" (p. 20), as "basal cartilages" (p. 23)—a term readily confused with baseosts—as "endoskeletal supports" (p. 24).

The query "What's in a name?" so far as it applies to text-books, should be answered by the rule: *A single name is of value; it should be once clearly defined with its synonyms and then used consistently throughout.* A similar indecision is observed in the descriptions of the vertebral elements, especially in the description of the unpaired or ventral vertebral elements, which are termed "hypocentra" in describing the Fishes, "hypocentra or intercentra" in describing the Amphibia, "intercentra (hypocentra)" in describing the Reptilia, and "intercentra" pure and simple in the later portions of the work. Parenthetically it may be observed that, upon page 127, the paired ventral pieces of the vertebrae of the Branchiosaurian and Microsaurian vertebrae are termed "hypo-

centra"; this is possibly in deference to the views of Cope and Baur as to the homologies of these elements, but in the reviewer's opinion both these writers were mistaken, for if these parts are homologised at all with the elements of a compound vertebra, they must be considered as *pleurocentra*, both because of their paired structure, and of their relations to the neurapophyses and ribs.

A similar inconsistency, which is perhaps even more serious, between the earlier and later pages of the work, is seen in the use of the terms "squamosal" and "supratemporal"; all through the portions relating to the Teleostome fishes and Stegocephalia, the dorsal anterior element immediately behind the post-orbital is termed "squamosal," while the ventral or posterior element is termed "supratemporal." In the latter part of the work these terms are reversed, and the ventral posterior element in contact with the quadrate is properly termed "squamosal"; while the dorsal anterior element in the Theriodontia, Pythonomorpha, and most subsequent sections is properly termed "supratemporal" or "prosquamosal" (Fig. 96, p. 153; Fig. 117, p. 192). As pointed out by Baur, these bones were first named by Owen in this manner, and it is obvious that the part nearest the quadrate should throughout be termed "squamosal," because the term originated in mammalian anatomy, in which there is no prosquamosal or supratemporal element.

Another debateable use of terms, which concerns homology, is that upon page 125, in the description of the dermal bones of the pectoral girdle of the Stegocephalia, where mention is made only of an interclavicle and clavicles; the former being regarded by the highest authority, Gegenbaur, not as an interclavicle at all, but as an episternum (that is, a part which originates by the union of dermal elements in the middle line of the chest, just as the parasternum or abdominal ribs originate in the region of the abdomen). There is thus in the Stegocephalia an episternum, a clavicle and a supraclavicle (Seeley = cleithrum, Gegenbaur), and this latter element, which the author fails to mention, is of especial interest because it represents a vestige of the ancestral suspension of the pectoral girdle from the skull, as observed in the Teleostomi. A minor oversight is in the absence of definition of the term "amphistylic" (p. 33), while of more importance is the absence of definition, either in the text or in footnotes, of the important ordinal and subordinal terms, such as "haplistia," "actinistia," "rhipidistia," terms originally applied in full recognition of their phylogenetic significance, and therefore worth fixing in the minds of students by a brief reference to their meaning.

The figures for the most part are extremely good, especially the original synoptic series drawn from Amphibian and Reptilian skulls in the British Museum. They are marred only by the abbreviations, which coincide with the inconsistencies in the use of terms, above pointed out. The Mammalian figures are the least satisfactory because the author has been obliged to borrow more largely. On page 295 is inserted Marsh's restoration of *Coryphodon*, which is to be regretted, because it gives an entirely wrong impression of the chief characters of this animal. Here also we regret the use of the term *Dinoceras*, which is pre-occupied by *Uintatherium*.

It is interesting to turn from these matters, which are of a more or less technical character, to the discussion of the far more important treatment of the relationships, phylogeny, and classification of the Vertebrata. Naturally in this part of the work specialists find their greatest interest.

The author, in the attitude of teacher, rightly takes a conservative position upon most of the open phylogenetic problems. He clearly shows that he leans towards the theory, that the closest relations of the Amphibia are with the Crossopterygian fishes, and of the Mammalia with the Anomodont Reptiles. In tracing the descent of the Mammalia proper, he adheres practically to the theory of Huxley, namely, of the phyletic succession of Monotremes, Marsupials, and of Placentals, the main objection to which is that no evidence can be advanced in its favour. He accordingly regards the Mesozoic mammals, and

especially the Purbeck and Como types as representing only Monotremes and Marsupials; and from this we infer that he considers the Eutheria or Placentals of comparatively late origin. A few errors have found their way into the treatment of the mammals, which may be corrected in a later edition. The teeth of Ancylopoda (page 307) are said to resemble those of the Rhinoceros. This is evidently incorrect, as they stand nearest the teeth of the Titanotheres, and hence of the Artiodactyls. Erroneously, we believe, the author follows Lydekker and Flower in placing all the early Perissodactyla among the Lophiodontidae, disregarding the fact that in the Lower Eocene all the modern families of Perissodactyls are sharply defined. This leads to a further confusion in reference to the family Palaeotheriidae, which is said to approach the Rhinoceroses. The conception of the family distinctness of early ungulates agrees with that held by the author himself in his treatment of the Fishes, namely, that family lines should correspond with phyla, and not with the parallel assumption by distinct phyla of similar characters.

One of the most remarkable things about this work is, that the author appears to be less clear and logical in the major classification of the Fishes, his own special field of work, than he is among the Reptiles. This appears to arise from far too great reliance upon the lines drawn by Reichert and Huxley between "autostylic" and "hyostylic" types, and from setting aside the really fundamental characters of division between cartilaginous fishes in which true bone is absent, and Teleostomes and Dipnoans, which have more or less true bone. Single characters, such as the suspensorium, may be safely employed in major classification, provided first that they are not acquired by parallelism, and second that they really afford sharp and natural lines of division. Hyostylism and autostylism answer neither of these criteria. The Elasmobranchs are grouped by the author as hyostylic (p. 27), although members of this sub-class show every stage of transition between autostylism (*Heptanchus*) and hyostylism (*Raja*). These transitions are obviously adaptations to feeding habits, analogous to those which we observe among reptiles with a loose or swinging, and tight or firm, articulation for the angle of the lower jaw. The autostylic *Chimaera*, with its firmly united upper jaw and reduced hyomandibular, is adapted to a mode of feeding upon small molluscs and crustaceans not very dissimilar from that of the lizard *Sphenodon*, in which the food is firmly seized by the anterior teeth and cut up by the molar plates. The loose and swinging shark jaws, with the hyomandibular acting as a supporting bar, are to be compared with those of snakes, in which the mouth is very expansible, and the food is swallowed whole, the backward-pointing teeth serving to prevent escape. Moreover, these terms are misapplied by the author not only in sharks, but in other specific cases. The Crossopterygians (p. 69) are defined as "hyostylic" (a character by the way which would seriously interfere with their position among the ancestors of the pronouncedly autostylic Amphibia), and this is an error, for, as observed by Pollard, "On examining the hyomandibular it will be seen that it can take very little part in the suspension of the jaws."

We thus conclude that a close examination of the suspensorium of all the living and extinct fishes proves that "auto-," "hyo-," and "amphistylism" are at the most ordinal characters, and cannot take rank as sub-class characters, except in the definition of the Dipnoi. Even then some of the orders are susceptible to adaptive modification, and thus do not present those sharp and fast lines of demarcation which are necessary for ordinal definition. To return to *Chimaera*, it is obvious that, so far as the concrescence of the palatoquadrate of the skull is concerned, this condition could readily be derived from that seen in the Notidani (*Heptanchus*) simply by fixation, and after carefully comparing all the other characters which are assigned by the author to give *Chimaera* and its allies a *sub-class* position, among the Holocephali, we find that they are no greater than those which give the Acanthodii and Pleuropterygii their *ordinal* position. Upon these grounds we conclude that the scheme of major classifica-

tion of the Fishes (page 27) is entirely unnatural, as it fails to set forth the phylogenetic relations of the Fishes, and confuses the student as to the types which are actually most nearly allied.

Passing to the Amphibia, it may be noted (p. 125) that among the chief distinctions of the Stegocephalia, the paired condyle is not mentioned. This is very important, because the condyle furnishes one of the only means of separating these animals from the Pareiasauria. Upon p. 142 the author gives a very clear conception of what at the present time appears to be the natural grouping of the Reptilia, but the tabular classification upon p. 141 hardly conforms with the conceptions there set forth. It appears to the reviewer evident that the Pareiasauria, with their solid skulls, entirely lacking temporal fossae (except perhaps in *Procolophon*), should be sharply set apart from the Anomodontia. As regards the use of the term Anomodontia in preference to Cope's "Theromorpha," it may be pointed out that it was originally defined and repeatedly used by Owen to include the Dicynodonts only, and it can scarcely be stretched to include the Theriodonts, with their complete dentition, and much less the Pareiasauria with no arcades at all. In the diagrams and subsequent treatment of the temporal arches, the lower arch is referred to as if it represented a concrescence of both upper and lower arches. This is certainly the case in the Ichthyosauria, but not so certainly in the other types. It appears undesirable to perpetuate Cope's term "Pythonomorpha," because Cope wholly failed to substantiate his claim in opposition to Owen, that these animals were in any way related to the Ophidia. Their entire structure tends to support the theory of Owen, Dollo, and Baur, that they branched off from well-defined Lizards quite independently of the Snake phylum.

Another surprising feature in this work is the entire absence of mention of the important group of Placodontia, animals which are ordinarily placed among the anomodont reptiles, fine examples of which are found in the British Museum. No reference is made either to *Aëtosaurus*, important and interesting because it shows the complete dermal armature of the Belodont type. It is important to note, however, that the author throughout his treatment of the Reptilia shows a very clear and critical perception of the relations of different types, and makes many original suggestions of value. While retaining *Belodon* among the Crocodilia, he strongly hints at its relations with the Dinosauria, as observed by Fraas, and one cannot compare the fine remains of *Belodon* and *Zanclodon* in the Stuttgart collection without being struck by the strong resemblance between the two.

It should be made clear, in closing, that the above discussion of the work has hardly left space in which to point out its many valuable qualities. It will find its way into every laboratory where Vertebrate Anatomy is being studied, and will serve to emphasise more strongly than ever the principle for which Huxley and Cope so long contended, that Vertebrate Anatomy is only comprehensible by the light of Palaeontology: that the only difference between the fossil and recent animal is that one has been dead longer than the other.

H. F. O.

The Rabbit. By J. E. Harting. 8vo, pp. viii. + 256. Illustrations. London: Longmans, Green, and Co., 1898. Price 5s.

This volume of the "Fur, Feather, and Fin Series" is a welcome addition to the literature of natural history and field sports. It will readily find its place on the book-shelves of sportsmen and naturalists, and will be of much service to gamekeepers and others interested in this useful though mischievous rodent. Much ignorance prevails, even amongst those who ought to know better, regarding the habits and peculiarities of the rabbit, and the intricacies of the law in dealing with the drastic provisions of that ill-advised and dis-

appointing enactment, the Ground Game Act. All needful information can be easily got from Mr. Harting's book, which is well indexed. The author has also much to tell us about rabbits attacking stoats, carrying their young, swimming across water, barking and killing trees, and so on, but space forbids our entering on these topics. A list is given of ornamental shrubs for undercover which are asserted to be "rabbit proof," and which "will thrive under the drip of trees." This is invaluable, and all sportsmen should see that this volume is put into the hands of their foresters and gamekeepers.

Mr. Harting enlarges, we think unnecessarily, on rabbits biting. Few sportsmen are ever bitten, for the simple reason that they do not handle many maimed rabbits. It is otherwise with gamekeepers, who handle thousands in taking them out of traps, and we have more than once seen them severely bitten. Again, it is asserted that tame rabbits will not burrow. This is erroneous, for we once turned out a black and white doe in a sandy burrow, where for several years she reared many litters. When a ferret was put in, she always bolted at once, and on two occasions turned and attacked her pursuer when it came outside. Being tame, she never attempted to run far away, and was often witness to her progeny bolting from the ferret, and being shot.

On the price of rabbits Mr. Harting says—"Trapped or snared rabbits should realise, say, 2s. 9d a couple; shot ones, 2s. 8d. a couple." We should much like to know where these prices can be got, as we can undertake to send heavy weekly consignments.

In referring to stoats and weasels, we fear Mr. Harting panders to sentimentalists, for he "would not ruthlessly trap and kill every weasel and stoat on the warren," and he faithfully reproduces the old story, which we had regarded as exploded, about "improving the stock by killing the weakly ones." As a corroboration of his statements he refers to the vole plague of 1892, and says—"Beyond doubt the weasel is the natural enemy of field-mice, and no greater mistake can be made than to destroy the former when the latter are numerous, or threatening to become so." As with lemmings in other countries, there were plagues of voles long before the days of game preservation or the trapping of weasels, and we venture to affirm that these plagues will occur again. The voles attracted all kinds of enemies: short-eared owls gathered and bred in their midst, and "blackening trains" of rooks found abundant food. As in previous visitations, the plague suddenly disappeared as unaccountably as it came, and we fear that Mr. Harting's weasel theory will not hold water.

In treating of long netting, after describing the process of setting the net, Mr. Harting says: "One or more helpers, as the length of net may require, stays back to extricate the rabbits from the net, or to knock them on the head with a stick as they try to force their way through." Nothing can demonstrate more clearly that the author has never seen the long net used, and we fear his information has been acquired from an unauthentic source.

We have noticed these points, but it would be gross injustice to suggest that they outweigh, or even overshadow, the value of the work. It is a delightful book, and one of genuine usefulness. The illustrations are carefully done, and the whole form of the book does credit to the publishers.

TOM SPEEDY.

## A STUDENT'S ASTRONOMY.

A New Astronomy. By Prof. DAVID P. TODD, M.A. Ph.D. 8vo, pp. 480, with figures. New York, Cincinnati, Chicago: American Book Company, 1898. Price 7s. 6d.

Prof. Todd is to be congratulated on having given to astronomical science a most valuable book, the need of which has certainly been felt by many teachers of elementary astronomy. There is nothing that tends so much to

promote a lively interest in the mind of the student as the practice of getting results from experiments of his own, and it is on this true principle that the author has based a great part of his work. Wherever possible, he suggests practical observations which, while requiring only the simplest instrumental equipment, naturally lead to a clear comprehension of the problems. The author's explanations are adapted to beginners, and the student is taught throughout to think for himself, instead of simply burdening his memory with disconnected data. It is safe to say that any one who will carefully follow this able guide, will become acquainted not only with the most interesting results of astronomy, but also with the methods which are required to deduce these from observation. An extensive series of original illustrations form a valuable feature of the book, which can be cordially recommended to all who are interested in the teaching of astronomy. J. HALM.

## EXPERIMENTAL CYTOLOGY.

La Cytologie expérimentale. Essai de cytomécanique. By ALPHONSE LABBÉ, Docteur ès Sciences, conservateur des collections de zoologie à la Sorbonne. 8vo, pp. viii and 187, with 56 figures. Paris: Carré et Naud, 1898. Price 5 francs.

As every biologist knows, cytology has ceased to be merely observational, and has become definitely experimental. But the literature embodying the advances which have been made by this new departure is scattered, and in some cases not very accessible. Hence the value of this compact little book, which supplies a clear and terse introduction to the subject, which may be read at a sitting. Dr. Labbé, who has himself made some valuable contributions to cell-lore, has here brought together illustrative examples of the most important concrete results of recent work on the influence of environment upon the cell, the inter-relations of nucleus and cytoplasm, "tropisms" and "tactisms," experimental embryology, and the like. There is no attempt at exhaustive treatment, which would have spoiled the book, and there is an unusual reserve of speculative suggestion, which is probably also wise. The book aims at being a student's introduction to the facts of the case, and as such it is admirable.

J. A. T.

## BIOLOGICAL LITERATURE OF SERVIA.

It is not generally known that Servia possesses four media of scientific thought. Two of these are found in the Srpska Kral'evska Akademija, of Belgrade, which publishes proceedings called *Ghlasa* in octavo, and *Spomenik*, transactions, in quarto. The word Ghlasa corresponds to the German Nachricht and Spomenik to the German Denkschrift. Both have been running since about 1888. The third is *Nastavnik*, the organ of the Servian professional society, a journal similar to the now defunct *Science Progress*, consisting of original articles only; while the fourth and last is *Delo*, or Journal of Literature and Science. A set of the first three, in so far as they pertain to mathematical and biological matter, has recently been received by the British Museum (Nat. Hist.).

## RECENT LITERATURE ON THE FORAMINIFERA.

Among the noteworthy papers which have recently appeared are two which represent the first efforts of Croatian and Servian zoologists to deal with the subject. One of these papers, by Velimir Dezelic, is entitled "Foraminifera jadranskoga mora," deals with the Foraminifera of the Adriatic, and was published in the ninth volume of the *Glasnik Hrvats Narod. Drustva*, in 1896. Unfortunately we have not yet seen it. The other is a paper by P. S. Pavlovic,

"Prilogh poznava'u Foraminifera iz II. Mediteranskikh slojeva u Srbiji," and was published in the *Ghlasa Srpske Kral'evske Akademije*, pt. lvi. 1898. This deals with the Mediterranean deposits near Belgrade, and figures are given of some interesting Frondiculariae. Another paper is by Detlev Lienau of Königsberg, and reads "Fusulinella, ihr Schalenbau und ihre systematische Stellung" (*Zeitschr. Deutsch. Geol. Ges.* 1898). Dr. Lienau comes to the conclusion that *Fusulinella* is a true calcareous Foraminifer, and that it does not belong to the Endothyridae, as stated by Neumayr and Rhumbler. An elaborate paper has also recently appeared in *Palaeontographica*, vol. xliv., by Dr. Schellwein, dealing with the Alpine *Fusulina* limestone. Numerous forms are figured and described, falling into many new species. Eight plates accompany the text, of which six represent the genera *Fusulina* and *Schwagerina*. A general paper, by Charles Upton, on "Chalk under the Microscope" (*Proc. Cotteswold Nat. Field Club*, vol. xii. pt. 3, 1898), treats of the contents of a piece of upper chalk from Purley, and is accompanied by an excellent plate of various forms.

"The Lower Cretaceous Gryphaeas of the Texas Region." By R. T. Hill and T. W. Vaughan. (*Bull. U.S. Geol. Surv.*, No. 151, 8vo. 1898). Pp. 66, 35 pls.

This paper treats of the species, stratigraphical occurrence, and relationships of one particular group out of the many kinds of fossil oysters found in the lower cretaceous formations of Texas. The group includes those forms which have been referred to *Gryphaea pitcheri*, Morton, and is of great importance owing to the assistance which a knowledge of the forms composing it renders in determining the true horizon of certain beds in geological sections.

Unfortunately inadequate descriptions based upon imperfect specimens, the true horizons of which had not been recorded, coupled with the loss for a long period of the type specimen of *G. pitcheri*, Morton, resulted in a fearful confusion of nomenclature, and gave rise to much controversy both as to the species and the beds in which it occurred.

From this condition of chaos the authors of this most carefully written memoir have extricated the subject. They show that under *Gryphaea pitcheri* no less than eight species have been included, and amongst them the familiar upper chalk *G. vesicularis*. Six members of the group, including two new species, belong to the lower cretaceous. The type species itself proved to be identical with *G. corrugata*, Say, which name consequently stands.

All these forms are fully described and figured, their geological distribution and probable phylogeny is traced, and the only point which a preliminary investigation suggests as wanting is some allusion to the late Félix Bernard's papers on the morphology of the hinge when dealing with the nepionic stage of *G. Washitaensis*. This last-named species, by the bye, calls to mind the well-known *G. vesicularis* of the upper cretaceous, both of Europe and America, and was doubtless its lineal ancestor. Many of the lower cretaceous forms here figured present apparent affinities to European species, though how far, if at all, the authors have studied the latter does not appear.

Anyhow much more work remains to be done on this difficult group of Ostreidae before satisfactory conclusions can be reached concerning them. To begin with, as the authors point out, "nearly all of the species of fossil oysters have been founded upon adult forms, without knowledge of the life-history of the individual. . . . It is regrettable that full suites of specimens representing all the species hitherto described have not been studied." In contradistinction to accepted authorities the observations of the writers of this monograph lead to the conclusion "that certain forms of the Ostreidae possess very distinct specific characters, have definite geologic horizons, and are $(BV)^2$ of the greatest value in stratigraphic work."

The authorities of the British Museum do not send out their "Catalogue of Birds" for review, but we must notice the completion of this great undertaking by the recent publication of volume xxvi. by Dr. Bowdler Sharpe and Mr. W. R. Ogilvie Grant. This is really the twenty-seventh volume issued since the work was begun twenty-five years ago. This catalogue, prepared by eleven authors and edited by Dr. Bowdler Sharpe, gives an account of 11,614 species belonging to 2255 genera and 124 families. Such additions and corrections as may be necessary will be published in supplementary volumes.

The Geological Society of London has undertaken the publication of the manuscript in its possession of a portion of the third volume of Hutton's "Theory of the Earth." This volume, edited by Sir Archibald Geikie, will be printed in the style of the first and second volumes of the same work, and will contain about 300 pages. It will be issued in paper covers. The price to the public is 3s. 6d., and as only a limited number will be issued, intending purchasers should apply at once to the Secretary.

As No. 25 of its publications, the Geological Survey of Norway has issued a geological map, with description by K. O. Bjørlykke, of Christiania and environs. This reproduces in more popular form the recent researches of Kjerulf and Brøgger, and may be of some use to the geologically-minded tourist. This Survey has also issued an account of its exhibits at Bergen in 1898, with which are combined articles of more permanent value on the Geological survey, the mining, and stone industry of Norway.

The first number of *Science Work*, the advent of which was noted in *Natural Science* for January, is chiefly noticeable for its list "of the leading contents of recent scientific magazines and periodicals"; the list is classified under the various sciences. For some reason the names of most of our own contributors are misspelled; indeed misprints of like nature are not rare throughout. Useful features are the first instalments of a "Directory of Local Societies" and a "Directory of Lecturers." The editor, Mr. Waller Jeffs, evidently means to make this new venture as successful as his firmly established journal, *Business*.

In the *Verhandlungen der russischen mineralogischen Gesellschaft*, 1898, Bd. xxxv. pp. 1-54, Th. Tschernyschew has a paper on the fossil sponges from the Permo-Carboniferous (Artinsk) and Carboniferous beds of the Ural and Timan. The paper, which is in German, is well illustrated by five plates and many figures in the text, mostly photographic. The sponges belong to the genera *Pemmatites*, *Kazania*, *Haplistion* (?), and *Stuckenbergia*, the last being new. In the sponge-fauna, as well as in other characters, the author finds a resemblance between the Artinsk beds and the upper horizon of the Permo-Carboniferous rocks of Spitzbergen, the *Productus*-bearing chert.

The Palaeontographical Society's volume for the year 1898 has just been issued, and contains—The "Palaeozoic Phyllopoda," Part III., by Professor T. Rupert Jones and Dr. H. Woodward, with 8 plates; the "Carboniferous Lamellibranchiata," Part III., by Dr. Wheelton Hind, with 10 plates; the "Inferior Oolite Ammonites," Part X. (Supplement No. I.), by Mr. S. S. Buckman, with 4 plates; the "Carboniferous Cephalopoda of Ireland," Part II., by Dr. A. H. Foord, with 10 plates; the "Devonian Fauna of the South of England," Vol. III. Part III., by the Rev. G. F. Whidborne, with 17 plates. The volume for 1899 is in preparation, and will contain the commencement of a new monograph on the "Mollusca of the Cretaceous Formations," by H. Woods.

# OBITUARIES.

## G. H. THEODOR EIMER.

Born 22nd February 1843; Died 29th May 1898.

Dr. G. H. Theodor Eimer, Professor of Zoology and Comparative Anatomy at Tübingen, was born at Stäfa, in the Canton of Zürich, where his father was district medical officer. His earliest years were passed at Freiburg, in Breisgau. At twelve years of age he entered the Gymnasium of Bruchsal. On leaving the Gymnasium he became a student in the medical faculty of the University of Tübingen, where he was in zoological subjects a pupil of Leydig. In 1863-64 he studied at Freiburg, in 1864-65 at Heidelberg, and in the latter year passed the examination in Natural Science at Karlsruhe. In the following winter he was again at Tübingen, and thence he went to Berlin to study the practical subjects of medicine. During the two years he was at Berlin he came much under the influence of Virchow. He graduated at the German metropolis in medicine and surgery in 1867, and in the following year passed the state examination at Karlsruhe. He had formed the resolution of devoting himself to research instead of practising the medical profession, and, on the advice of Virchow, went again to Freiburg to work in Weismann's laboratory. Here his interest in the problems of evolution, already aroused, was strongly developed, and he commenced the investigations which led him to views in many respects diametrically opposed to those of his teacher. In 1869 he acted as Prosector of Zootomy to Kölliker in Würzburg, but in 1870 he gave up academical pursuits for a time, and the day after his marriage set out with his bride for the seat of the Franco-German war. He took part in the campaign as volunteer field-surgeon, while his wife was a volunteer field-nurse. The hardships of the campaign enfeebled his health, and in 1871 he went to Italy to recuperate. During this and subsequent visits to Italy he carried out his well-known researches on Medusae and other marine forms, and on the variations of the lizard. On his return to Würzburg in 1871 he set up as privat-docent in zoology. In 1874 he became Inspector of the Grand Ducal Museum at Darmstadt, and Professor of Zoology at the Polytechnic High School of that city, and in the following year was invited to succeed Leydig as Professor of Zoology at Tübingen in the University in which he commenced his academical career.

Although he made various contributions to histology and other subjects, Eimer's most important investigations are those which deal with problems of evolution. He has taken a prominent part in the tournaments waged over these questions during the last two decades. Not entirely neglecting other phenomena, he gave his attention principally to the external characters which distinguish species and genera from one another in various groups, and has contributed powerfully to the growing conviction that such characters are not to be explained on the principle of simple utility. In his work on *Lacerta muralis*, and his papers on the markings of mammals published in "*Humboldt*" between 1878 and 1888,

he pointed out the evidence that changes in markings occur in a definite order, and are excited by change of conditions. He was strongly convinced that variation occurs, not in all directions, but in a limited number of definite directions, in other words that the direction of variation is largely pre-ordained by the constitution of the organism. In 1888 he published a summary of his facts and conclusions under the title "Entstehung der Arten, auf Grund vom Vererben erworbener Eigenschaften, nach den Gesetzen organischen Wachsens." He followed this up by works on the characters of butterflies, in which he elaborated his method of investigation in great detail. The first of these, in two parts (1889 and 1895), referred to the species of the genus *Papilio*, and allied forms. His latest work, "Orthogenesis der Schmetterlinge," was published in 1897, and contains a mass of important and interesting detail.

Eimer's powers of lucid exposition were not perhaps of the highest order, and his methods of controversy were often too personal and passionate. But on the other hand his researches are very original, penetrating, and illuminating, and his bitterness, however much to be regretted, was not unnatural. It is unlikely that the laws he was fond of formulating will prove to be of general application, but his investigations of diagnostic characters, as for example in the case of leaf-like butterflies, bring to light important phenomena whose significance had not previously been perceived, and he has left behind him a body of work which will be of permanent value in the pursuit of one great object of biological research, namely, the discovery of the particular causes of the characters of organisms. J. T. C.

The deaths are also announced of Mr. JOHN BARROW, F.R.S., who died in Dec. 1898 at the age of 91—he took a leading part in promoting the search for Sir John Franklin; on May 13, 1898, at Stockholm, Dr. SVEN BORGSTRÖM, bryologist, aged 72; Dr. THOMAS SANDERSON BULMER, by suicide, at Sierra Blanca, Texas, on Oct. 5—a student of American archaeology and ethnology; on Aug. 14, 1898, at Taftville, Conn., Prof. JOHN COMFORT FILLMORE, ethnologist, of Pomona College, California; CAMILLE FLAGEY, lichenologist, in Algiers, aged 62; JOSEPH GIBELLI, Professor of Botany at Turin University; the entomologist, LOUIS A. GLASER of New Brighton, at Mannheim, Germany; on Dec. 21, ALFREDO ANTUNES KANTHACK, Professor of Pathology in Cambridge University, aged 35; Mr. CHRISTOPHER YOUNG MICHIE, a leading Scottish authority on forestry, author of *The Larch*, *The Practice of Forestry*, etc.; on Oct. 23, Dr. HERMANN MÜLLER, privat-docent for bacteriology at Vienna, aged 32; JACQUES PASSÉ, an assistant in the laboratory of physiological psychology at the Sorbonne, well known for his researches on the sense of smell; Dr. AUGUST POLLMANN, teacher of agriculture at the Agricultural Academy, Bonn, on May 16, 1898, aged 85; on Aug. 6, at Irrenhaus, EMERICH VELLAY, formerly assistant at the Hungarian entomological station; G. VENTURI, the Austrian bryologist, on June 5, 1898, bequeathing his moss herbarium and library to the city of Trent; on Jan. 15, Mr. ANDREW WILSON, L.D.S., late Vice-President of the Royal Physical Society, Edinburgh —an expert odontologist and an enthusiastic student of Mammalian dentition and Lepidoptera. We regret also to have to announce that Dr. Henry Alleyne Nicholson, Professor of Natural History in the University of Aberdeen, died after a short illness, on January 19, at the age of fifty-five. An obituary notice will be given next month.

# CORRESPONDENCE.

## THE AUTHORSHIP OF ILLUSTRATIONS.

The very just note in your December number, pp. 365-367, leads me to ask, Where does the artist come in, supposing he is not the author? Walker, years ago, described some Diptera in a more or less unintelligible manner; but Westwood prepared the illustrations, which were very excellent, and served for the recognition of the species. Now should these illustrations be credited to Walker? Ashmead, in the *Canadian Entomologist*, 1898, p. 309, has the following note—"*Allomorpha* [a genus of saw-flies] may be wrongly placed in my table. Cameron says nothing about the venation of the hind wings. His description reads—'Alar neuration resembles *Strongylogaster* (*Cingulatus* group).' Now, *S. cingulatus* has two discal cells in the hind wings. Kirby, however, who, I believe, examined the type, says—'*Hind wings with one discal cell.*' The artist, however, employed by Kirby has figured it on Plate X., No. 22, *without a cell in the hind wings!* In my perplexity I have followed the artist, since I find his figures of saw-flies known to me perfectly accurate."

Now, if the artist referred to by Ashmead is better authority than Kirby or Cameron, so that when a difficulty arises his figure is assumed to be correct rather than their descriptions, does he not deserve to be quoted as an author? Of course, the artist does not necessarily possess any technical or critical knowledge of the objects he draws, nor can he be made responsible for the labelling of his figures. At the same time, it is probable that if the scientific artist received more recognition for his work, that work would be better done. I do not think that at present we always treat him fairly.

T. D. A. Cockerell.

Mesilla Park, New Mexico, U.S.A.

## TELEGONY.

Sir—After a careful study of the facts already ascertained regarding telegony—that peculiar phenomenon of cross-breeding, popularly termed "throw-back"—we see that these can be attributed to reversion with almost as much likelihood as to telegony. Nevertheless, we feel assured that telegonic effects are sometimes shown by the offspring; and as this question is of the highest importance to any theory of heredity, we seek through your columns to extend our appeal for co-operation in experiment, and suggest the following:—

A female of a wild species (*e.g.* the wild rabbit) should first bear young to a male of a domesticated breed of the same species (*e.g.* the Angora rabbit), and then to a male of her own breed. If, then, the progeny of this union were to show any traces of the first union, they must be due to telegony; for, of course, no type can revert to one more differentiated than itself. Birds might be experimented on in a similar way, the first crosses being, *e.g.*—the "barn-door" cock—pheasant hen; a domesticated drake—wild duck.

We are ourselves conducting as many experiments as we can, but telegony is without doubt of extreme rarity, and the more trials that are made the greater is the chance of success. Those who will assist are asked if they will kindly send the skins of those parents and young which display telegony; also, in the case of birds, to send an egg out of each batch.—We are, etc.,

Alex. Meek.

Durham College of Science, Newcastle-on-Tyne.

G. P. Bulman.

Cullercoats, Northumberland.

# NEWS.

The following appointments have recently been made:—Elmer D. Ball, to be assistant entomologist at the Colorado Experiment Station; Dr. J. Behrens, of Carlsruhe, to be bacteriologist to the newly-founded biological section for agriculture and forestry at Berlin; J. P. Blanton, to be president of the University of Idaho, and director of its Experimental Station; Dr. L. Bordas, to be chief of zoological work in the faculty of science at Marseilles; Antonio Borzi, to be professor of zoology and comparative anatomy at Palermo University; Mr. Thomas Bowhill, F.R.C.V.S., author of "Manual of Bacteriological Technique," to be bacteriologist to the Glamorgan County Council; Karl Brischke, as director of the Botanical Garden in Thorn; C. W. Burkett, to be associate professor of agriculture at the New Hampshire College; Dr. John M. Clarke, to be state palaeontologist of New York; Dr. Dannenberg, to be professor of mineralogy and geology at the Mining Academy in Clausthal; Prof. J. R. Ainsworth Davis, as additional examiner in zoology in the University of Edinburgh; Dr. Rudolf Disselhorst, to be professor of animal physiology at Halle University; Dr. Eugen Dubois (*Pithecanthropus*), to be professor of geology at Amsterdam University; Dr. E. P. Felt, to succeed the late J. A. Lintner, as state entomologist of New York; M. T. French, to be professor of agriculture at Idaho College and Experimental Station; Max von Frey, from assistant professor to full professor of physiology at Zürich University; Mr. Gravier, to be assistant in the Paris Museum of Natural History in the place of the late Félix Bernard; Dr. D. Frazer Harris, to be lecturer in physiology at St. Andrews University; Dr. Hettner, of Tübingen, to be professor of geography at Würzburg University; Dr. G. Bertram Hunt, as assistant in the department of pathological histology at the University of London; Dr. Ernst Kalkowsky, to be director of the mineralogical, geological, and archaeological collections at Dresden; Dr. Klockmann, of Clausthal, to be professor of mineralogy and geology in the Technical Institution at Aix; Dr. Kolkwitz, to be professor of histology at Innsbruck University; Dr. Casimir Kwietniewski, as assistant in the cabinet of zoology and anatomy at Messina University; Dr. Lambert, as assistant at the plant-physiology experimental station in Geisenheim; Dr. Lepetet, to be professor of histology in Clermont University; Dr. James Little, as Regius professor of medicine in Dublin University; Alberto Löfgren, to be director of the botanical gardens at São Paulo, Brazil; Charles P. Lounsbury, of Amherst, Mass., as government entomologist at Cape Town; Dr. R. Stewart MacDougall, as examiner in agricultural entomology in the University of Edinburgh; Dr. de Marignac, to be professor of hygiene at Geneva University; Dr. F. J. H. Merrill to be state geologist of New York, the palaeontologist work being taken by Dr. Clarke (see above); A. S. Miller, to be geologist to the Idaho Experiment Station; Dr. M. von Minder, as assistant in botany at Giessen University; Dr. Heinrich Obersteiner, to be full professor of the physiology and pathology of the central nervous system at Vienna University; C. S. Parsons, as director of the Arizona Agricultural Experiment Station

in Tucson; Prof. G. Perroncito, as director in the Royal Veterinary School of Turin; Dr. G. N. Pitt, of Guy's Hospital, as examiner in medicine at Cambridge University; Dr. Fritz Roemer, of Jena, as assistant in the zoological department of the Museum für Naturkunde, Berlin; Dr. D. Saccardo, to assist Prof. F. Morini in the botanical garden of Bologna University: W. G. Savage, M.B., as assistant in the department of bacteriology at University of London; Dr. Fritz Schaudinn, as privat-docent for zoology in Berlin University; Dr. Schroeter, as privat-docent for botany in Bonn University; Dr. L. S. Schultze, as assistant at the Zoological Institute of Jena University; Dr. Carl Freiherr von Tubeuf, of Munich, to be director of the botanical laboratory in the Biological Experiment Station for Agriculture and Forestry at Berlin; J. A. Thomson, as additional examiner in zoology in the University of Glasgow; Alexandro Trotter, as assistant in the botanic garden of Padua; Dr. R. Wagner, of Munich, as assistant in the Institute for Vegetable Physiology at Heidelberg; Dr. Karl Wehmer, to be professor of mycology at the Technical College in Hannover; Dr. N. Wille, to be curator of the Museum and Herbarium of the University of Christiania; Jas. Withycome, to be assistant director and agriculturist of the Oregon Experiment Station; Dr. R. W. Zimmermann, to be professor-extraordinarius of anatomy at Berne University; Dr. Oskar Zoth, to be professor-extraordinarius of physiology at Graz University.

Among the New Year's honours we notice a baronetcy for the surgeon Sir Sir Henry Thompson. Prof. W. C. Roberts Austen, of the Mint, is promoted to be K.C.B., and Mr. Thiselton Dyer, the director of Kew Gardens, to be K.C.M.G., in recognition of services rendered to Colonial Governments. Pure science, it will be seen, meets with no recognition, yet see *Natural Science*, vol. xiv. p. 1.

Mr. William Christopher MacDonald, merchant, of Montreal, has received the honour of knighthood in recognition of his gifts to educational and philanthropic objects in Canada. He is said to have given to M'Gill University about 1,600,000 dollars in all.

The Royal Academy of Sciences, Stockholm, has awarded its Linnaeus medal to Mr. J. Stadling, in recognition of his journey through Siberia and a gift of fish from that country.

On occasion of the Jubilee of the St. Petersburg Academy of Medicine, the following British scientists have been appointed honorary members of the Academy: Sir William MacCormac, Sir William Turner, Lord Rayleigh, Sir William Stokes, Drs. William Macewen, Thompson, and Branston.

The Council of the Royal Institute of Public Health has awarded the Harben Gold Medal for 1899 to Lord Lister, P.R.S., in recognition of his eminent services to preventive medicine; and it has appointed Professor William R. Smith, M.D., D.Sc., as Harben Lecturer for the year 1899; he has chosen "Diphtheria" as his subject.

Sir John Lubbock has been elected a member of the Royal Society of Science at Upsala.

During the past year the total number of matriculated students attending the University of Edinburgh was 2813, including 211 women. Of this number 817 (including 190 women) were enrolled in the Faculty of Arts, 147 (including 5 women) in the Faculty of Science, 63 in the Faculty of Divinity, 373 in the Faculty of Law, 1387 (including 6 women) in the Faculty of Medicine, and 26 (including 10 women) in the Faculty of Music. Of the students of medicine, 584, or over 42 per cent, belonged to Scotland; 374, or nearly 27 per cent, were from England and Wales; 94 from Ireland, 60 from India, 231, or fully 16½ per cent, from British Colonies; and 44 from foreign countries.

The Regents of the University of the State of New York have decided to divide the work in geology and palaeontology, which was for so many years in charge of the late Professor James Hall, and in so doing have erected two co-ordinate departments, one of palaeontology and stratigraphic geology and the other of "pure geology," the latter to cover dynamic and physical geology, the crystalline rocks, superficial geology, etc. Some of the changes are noted in our list of appointments.

*Science* announces that Dr. Thomas Egleston, emeritus professor of mineralogy and metallurgy in Columbia University, has presented the university with his library and mineralogical collection, the latter containing about 5000 valuable specimens.

We learn from the *American Naturalist* that the biological and geological departments of the Massachusetts Institute of Technology have moved into new and more spacious quarters in the Pierce building recently erected.

The King of Sweden and Norway has given the Swedish Academy of Sciences the sum of about two hundred guineas for continuing the investigation of Swedish seas under the direction of Professor Pettersson. During the present year it is intended to continue the soundings in the Baltic and Gulf of Bothnia, and the continuous observations in the Skagerrak; also to investigate the conditions on the northernmost parts of the North Sea plateau, where is the fishing-ground of the Swedish bank-fishers, and the conditions during the months of October and November, after the close of the herring-fishery.

The Royal Academy of Medicine of Belgium proposes to ask the Belgian government to enter into negotiations with foreign governments, with a view to drawing up an International Pharmacopoeia.

The awards of the Geological Society of London for the year 1899 will be as follow:—Wollaston Medal, Charles Lapworth; Wollaston Fund, J. B. Harrison, of British Guiana; Murchison Medal (this year awarded twice), Benjamin N. Peach and John Horne; Murchison Fund, James Bennie; Lyell Medal, General C. A. M'Mahon; Lyell Fund, divided between Frederick Chapman and John Ward; Bigsby Medal, T. W. Edgeworth David, of Sydney.

The gold medal of the French Geographical Society has been awarded to Mr. Gentil for his explorations in the neighbourhood of Lake Tchad.

The Seventh International Geographical Congress is to meet in Berlin from Thursday, September 28, to Wednesday, October 4, of this year. The subscription for members is £1. All who wish to contribute papers are requested to give notice before April 1, and to send their MS. by June 1; abstracts, if desired, should not exceed 1500 words, and must be sent in before August 1. Motions to be laid before the Congress should be sent in not later than June 1. The languages admitted at the Congress are English, French, German, and Italian. All correspondence is to be addressed to 90 Zimmerstrasse, Berlin, S.W.

The Committee of the International Geological Congress, 1900, which will meet at Paris, has just issued, through the Minister of Commerce, Industry, etc., the first circular. The Congress will take place from the 16th to the 28th August 1900, after which will follow these excursions:—Tertiaries of the Paris basin (2 days); the Boulonnais and Normandy (10 days); the central massiff (10 days); the Ardennes (8 days); Picardy (6 days); Brittany (10 days); Mayenne (8 days); the turonian of Touraine and the cenomanian of Mans (6 days); Touraine (4 days); Morvan (10 days); coal-measures of Commentry and of Decazeville (7 days); Mont Dore, chaîne des Puys and Limagne (10 days); Bharentes (8 days); Bordeaux basin (6 days); Tertiaries of the Rhône and the Casse-Alps (8 days); Dauphiné and Mont Blanc (10 days); Hautes Alps (10 to 12 days); Monts Ventoux and de Lure (10 days); Basse-Provence (10 days);

le Montagne Noire (8 days); Pyrenees (10 days). A special geological guide will be ready early in 1900; meanwhile English geologists will remember the excellent "Geology of the Paris Basin," by Messrs. Harris and Burrows, published by the Geologists' Association of London.

The new president of the Anthropological Society of Paris is Dr. Capitan.

The new president of the American Microscopical Society is Dr. William C. Krauss, of Buffalo, N.Y.

The New York Zoological Society has issued No. 3 of its News Bulletin. This is illustrated with four large photographic figures, showing the works at present in progress. A great deal seems to have been accomplished since the summer, and it is hoped to have the Park partially stocked with animals, and open to the public, by May next.

It is announced that those interested in promoting Zoological Gardens in Edinburgh have fixed their eyes provisionally on a site in the neighbourhood of Barnton. As we pointed out before, the only hope of success, in default of abundant capital, is to choose a site in some habitual resort of holidayers, and this consideration points to Portobello rather than to Barnton.

The President of the American Society of Naturalists, at its New York meeting in December 1898, delivered an address on "Reform in Medical Education." This is published in *Science* for December 30. Eleven other scientific societies were meeting during the same period.

The Excursion Secretary of the Geologists' Association, London, is now Mr. F. Meeson, 29 Thurloe Place, South Kensington. He is trying to organise some cycle excursions.

The "At Home" of the President and Council of the Geological Society, London, on December 15, was attended by a large number of Fellows. With the aid of the lantern Mr. H. W. Monckton showed photographs of the glaciers and fjords of the Bergen district, Norway, and Mr. C. W. Andrews showed his photographs of Christmas Island. There were many interesting exhibits. The Director-General of the Survey produced markings from the Silurian rocks of Tipperary, simulating shells, graptolites, and fucoids, but probably due to inorganic causes. Mr. Whitaker exhibited a geological map of London and suburbs made in relief on stamped tin by J. B. Jordan. Mr. Etheridge exhibited cores from the Brabourne borehole in South-East Kent; this has now reached a depth of 2003 feet in Lower Carboniferous rocks.

Mr. Monckton redelivered the above-mentioned lecture to the Geologists' Association, London, on January 6.

The Preston Scientific Society held its annual meeting on November 23, 1898. It was announced that during the year the number of members had increased from 520 to 710. This being so, it is the more to be regretted that the Corporation should have found itself unable to continue to permit the Society the use of its rooms in Cross Street, causing the Society pecuniary loss, and the trouble of furnishing new rooms at 119A Fishergate. The collections of the Society are growing. Miss Robinson has presented a large collection of British Plants; while a series of local land and fresh-water mollusca has been given by Messrs. R. Garbet and W. H. Heathcote. The syllabus of lectures and sectional meetings for the current session is remarkably full, but contains rather a larger proportion of lectures on general subjects than we like to see in the case of an actively working local society. The new president is W. E. M. Tomlinson, M.P.; and the secretary is W. H. Heathcote, 47 Frenchwood Street, Preston.

The retirement of Sir John Evans from the treasurership of the Royal Society, after a period of service of twenty years, has seemed a fitting opportunity for

the Fellows of the Society to give expression to their sense of obligation for the efficient manner in which he has discharged the arduous duties of his office. There is a consensus of opinion that the most suitable form for marking appreciation would be a portrait painted in oil colours. This would find an appropriate place on the walls of the Society's apartments at Burlington House, where Sir John Evans has for many years past spent a considerable proportion of his time in carrying on the work of the Society. Subscriptions from Fellows of the Society will be received by the assistant secretary.

At a meeting of the Royal Physical Society of Edinburgh, on January 18, the following communications were read:—On the genesis of some Scottish minerals, part i., by Mr. J. G. Goodchild; on the minute structure of the so-called "rectal" gland of the skate, by Dr. John Crawford; and on spiders of the Edinburgh district (190 species now recorded), by Mr. W. Evans and Mr. G. H. Carpenter. Mr. Crawford contrasted the structure of the "rectal" gland with that of the appendix vermiformis, and noted especially the remarkable disposition of the vascular supply. A glycerine extract gave no evidence of digestive function.

At the Royal Victoria Hall, Waterloo Bridge Road, London, the following penny science lectures were delivered during January:—Rev. R. H. Whitcombe, "Science Jottings in Switzerland"; Mr. R. A. Gregory, "Astronomy before Telescopes"; Prof. Farmer, "Plants as Engineers"; Mr. C. W. Andrews, "Christmas Island"; Prof. Frank Clowes, "Old Father Thames."

During the present term Mr. A. G. Tansley is giving a course of about thirty-three lectures on the morphology and histology of the vascular system of plants at University College, London. Each lecture is followed by two hours' practical work.

The conflagration which recently broke out in the physical laboratory of the University of Geneva destroyed Prof. Chodat's botanical collection, together with 200 drawings by the professor, which were the result of ten years' labour.

In an article in the *Glasgow Herald* of January 7 Dr. Robert Munro refers to an article on the marine structure recently discovered in the estuary of the Clyde, which appeared in the November number of *Natural Science*, and in which his name is introduced as one who, after making a thorough investigation of the site, "declared it was the most curious, puzzling, and interesting find of the kind he had met with in all his long experience, and, so far as he knew, unique." From the general purport of that article the public may have supposed that the opinions there promulgated as to the age, structure, and marvels of the Dumbuck crannog have been endorsed by Dr. Munro. But this is not so, as he regards it neither as a pile-structure nor as a monument of Neolithic times. In attempting to solve the riddle of the remarkable art gallery—idols, amulets, and ornaments of shale and shell—there are two alternative conclusions to be formulated. Either these objects are what the investigators assert them to be, the genuine relics of the inhabitants of the fort and crannog, or they are not. On the former hypothesis they form the most remarkable collection of archaeological remains ever found in Scotland. On the latter, they are the productions of some idle practical joker.

Dr. Munro wishes to state that he has not the slightest clue to the *provenance* of the relics whose genuineness he calls in question, as his opinion is based entirely on their inherent character and total variance with all other archaeological remains known to him.

Dr. F. P. Moreno, of the La Plata Museum, exhibited at the Zoological Society of London, on January 17, a fragment of the skin of the animal described by Ameghino as *Neomylodon*. The piece was about 18 inches across either way. Dr. Moreno and Professor Seeley maintained that the fragment was probably a portion of a *Mylodon* which had been preserved under similar con-

ditions to the Moa skin and feathers of New Zealand. Prof. Seeley compared it with the skin of *Rhinoceros* and *Elephas* found in the tundras of Siberia, skin which he emphatically stated had never been found in frozen soil or in masses of ice. With this statement the meeting distinctly disagreed. The general opinion of those zoologists present, on the main question, however, was that the skin exhibited was not that of a fossil, but of a recent, or comparatively recent, animal. The fragments in Ameghino's possession Dr. Moreno believes to be from this identical portion of skin, and if that be so, then English zoologists will have little doubt in deciding that Ameghino is correct in assigning the discovery to a new form, named by him *Neomylodon*.

Although the Apothecaries' Society has decided to abandon the management of the Chelsea Physic Garden, it will not disappear under bricks and mortar. The control is to be vested in the Trustees of the London Parochial Charities, but it will be managed by a committee of fifteen, including representatives of the Royal Society, the Technical Education Board, the Society of Apothecaries and the Royal College of Physicians in turn, the Pharmaceutical Society, and the Senate of the University of London. The existing garden will be maintained, rooms provided for lectures and experimental teaching, and a physiological laboratory may be erected. An income of £800 is to be provided by the Trustees, and other sums will be provided to carry out all the details of the scheme. Instruction will be provided, material supplied for teaching, and the Royal College of Science teachers and students will be allowed the use of the garden and rooms.

We alluded in our last number to Mr. Frederick W. Christian's explorations in the Caroline Islands. *The Times* states that Mr. Christian stayed nearly three years in Samoa, studying the language and customs of the people, especially those farthest removed from the settlements of the white man. In Tahiti and the Marquesas he spent two years, minutely noting down the language, the genealogies, folklore, and traditions of the inhabitants. He visited single-handed Spanish Micronesia, in order to obtain some further and minuter information upon certain mysterious ruins reported to exist upon Bonate, or Ponape, and Lele, two islands lying further to the eastward of the extensive Caroline chain. Here he obtained some 150 photographs in the districts of Kiti, U, Metalanim, Not, and Chokach (wrongly styled Jekoits and Jokoits in the present charts). The walled islets of Nan-Matal were explored and mapped out fairly accurately. The phonesis of very many native names and their spelling were changed from a meaningless jargon to their correct native renderings and accompanying significations. He also made excavations in the central vault of the sanctuary of Nan-Tanach, bringing to light a considerable number of curious tools, implements, and shell ornaments of an ancient date. Many of the old native legends and fairy tales were rescued from oblivion. Some new information was obtained about the flora and marine life of the archipelago. The former presence of an early Negrito race, conquered and absorbed or overlapped by later waves of Polynesian, Malayan, and Melanesian immigrants was fairly established. Also evidence was collected as to the obtrusion of many Japanese words upon the Micronesian area. Mr. Christian spent three months on Yap, in the Western Carolines, where he found ancient platforms and burial-places of Japanese design. He proposes to revisit the Caroline and Marianne Islands, especially Ruk, Tinian, Saipan, Pulawat, and Nuku-Oro, also the Pelew Islands.

C. F. Baker, of the Alabama Experiment Station, left on January 1 for a collecting trip of a year and a half's duration in South America.

Prof. Alexander Agassiz is said to have gone to Rhodesia to investigate its mineral resources. It is safe to say that he will not confine his attention to these.

The U.S. Fish Commission steamer "Fish Hawk" has gone to Porto Rico for the winter. The party it carries will, says *Science*, make a careful study of the various forms of life in the waters about the island, and incidentally the fauna and flora of the land will be studied and collections made in various branches of natural history. The material will be submitted to specialists, and their united papers will form a comprehensive report on the natural history of the island. Professor B. W. Evermann is in charge, and he will be assisted on the part of the Fish Commission by Mr. H. F. Moore, E. C. Marsh, and others. Entomology will be cared for by Mr. August Busck, of the Department of Agriculture, while Mr. A. B. Baker will represent the National Zoological Park. Mr. A. H. Baldwin goes as artist.

Dr. Carl Aurivillius, of Upsala, left Stockholm on January 6 to renew his observations in Java, the Sunda Islands, and possibly Timor. He will make general zoological collections in this interesting but still little known region, and make special studies on the fauna of coral reefs. While on his voyage he will conduct researches on the plankton of the Indian Ocean and the sea around the Sunda Islands. The expense of this will be paid out of the Lars Hierta Memorial Fund; but the general cost of the expedition will be defrayed by the J. A. Wahlberg stipend (awarded for the first time) as well as the larger of the State travelling stipends.

Prof. A. C. Haddon writes to *Nature* that his expedition has now completed its work in Torres Straits. Dr. Rivers and Mr. Wilkin have left for England, while the other members of the expedition have proceeded to Borneo to study the anthropology of the Baram district of Sarawak. The health of the party has been excellent.

The natives of Murray Island were studied with most detail, as, owing to their isolation, they have been less modified by contact with alien races. Some of the party stayed about four months on the island, others only a couple of months, having made a trip to the mainland of New Guinea, where they visited the coast tribes between Kerepunu and the Mekeo district, and took several excursions for short distances inland. There was not enough time spent at any spot for a thorough investigation of the natives, but a considerable amount of information was obtained in most of the branches of anthropology with which the expedition is concerned; and this will prove of value for purposes of comparison. Over a month was spent in Mabuiag (Jervis Island) by all the party, with the exception of Messrs. Myers and M'Dougall, who had previously started for Borneo. Although the time spent in Mabuiag was short, a satisfactory amount of work was accomplished, owing to the conditions being favourable. Observations were also made on several other islands in Torres Straits and in Kiwai, which is situated at the mouth of the Fly River.

A large number of photographs have been taken, and considerable collections, which are now on their way to Cambridge, have been made.

Dr. C. F. Millspaugh, the botanist, and E. P. Allen, the photographer, of the Field Columbian Museum, Chicago, left New York on December 24 for the West Indies on Mr. Allison Armour's yacht "Utowana," in order to make as complete a collection of the flora as possible.

In a letter to *Aftonbladet*, Mr. Stadling of the Andrée Search Expedition, complains that his telegram from Yeneseisk was transmogrified beyond recognition, and gives the following account of his travels:—After 300 miles stormy voyage through the delta, they were frozen up on an uninhabited island in the sea west of the Lena delta and about 100 miles from the mouth of the Olensk. After seventeen days they were able to obtain dog-sledges from the interior and to proceed over the sea. Reindeer took them from the mouth of the Olensk over 300 miles of uninhabited tundra to Anabar, thence in a north-westerly direction near to the mouth of Chatanga Creek, and so southwards to Chatangskoje. Crossing the Taimyr peninsula, they took a more northerly direction

than the usual winter road of the hunters, so as to visit the natives, who were still out on the tundra. The route led over the watershed between the rivers Chatarya, Taimyr, and Pjasina, to Dudinskoje on the Yenesei, about 1800 miles from the Lena delta. Thence reindeer and horses took them 1200 miles to Krasorojarsk, where they took the train on December 2. None of the natives or wandering Dolgans and Samoyedes had any news of Andrée, though a few had heard tell of the expedition three years before.

Another Andrée rumour comes through Prof. Nathorst of Stockholm. Fourteen days before midsummer, the Norwegian whaler, "Harald Hårfager," was in the ice between Iceland and Greenland, in 66° 33′ N. and 28° W., when the crew of one of her boats saw on an ice-floe a large mass like a heap of brushwood or a pile of steel rails. Fog and the fear of losing their ship prevented a closer examination; and though the captain, when it was reported to him, lay to for several hours and then steered in the direction indicated, the mists prevented rediscovery of the curious appearance. It was not till later that this object was connected in their minds with Andrée's balloon. This, however, is a possible explanation, for, supposing the balloon to have met with mishap north of Spitzbergen, it would have drifted across to Greenland and then down its east coast.

The well-known traveller, Captain Daniel Brunn, has decided to organise an expedition next summer to search for traces of Herr Andrée and his companions in the neighbourhood of Eastern Greenland. The expedition will start from the east coast of Iceland and proceed by way of Jan Mayen Island, to the vicinity of Cape Barclay on the east Greenland coast.

The interesting results obtained by the expedition of Mr. J. E. S. Moore to Lake Tanganyika have led to the formation of a committee to organise another expedition for the purpose of thoroughly surveying the basin, not only of Lake Tanganyika, but also the northern extension of the series of valleys in which this lake, along with Lake Kiou and the Albert Nyanza, lie; of collecting specimens of the aquatic fauna and flora, and of studying the geological history of the region. The last-mentioned object is of special interest, considering the marine Jurassic character of the gastropods now living in the lake. The likelihood of the halolimnic fauna, as Mr. Moore calls it, being found in the lakes to the north is increased by the fact that it bears some resemblance to the fauna of the Lower Nile. The proposed expedition would go northwards from Tanganyika as far as the Albert Edward and Ruwenzori districts, then eastwards down the Uganda roads to the sea. The African Lakes Corporation is now running a steamer on Tanganyika, and this will permit of dredging and sounding. The Committee consists of Sir John Kirk, Dr. P. L. Sclater, Sir William Thiselton Dyer, Prof. Ray Lankester, and Mr. G. A. Boulenger. It estimates that at least £5000 will be required, and appeals for pecuniary support.

A telegram from Sydney has been received at the Royal Society, stating that the boring into the coral reef, or rather atoll, of Funafuti had been discontinued on reaching a depth of 1114 feet. Cores had been obtained, and the material traversed is described as "coral reef" rock. By comparison of this information with that previously received it would appear that the lower part of the mass pierced consists of a hard limestone, apparently reef material, a rather sudden change from softer and more variable stuff—a mixture of sand composed of calcareous organisms with reef coral—taking place at a depth just short of 600 feet.

Another attack on *Polypterus* in the Nile valley is to be made this year by Prof. E. B. Wilson of Columbia University.

Lord Lister and Sir Henry Roscoe announce that Lord Iveagh has offered the sum of £250,000 for the purposes of the highest research in bacteriology

and other forms of biology as bearing upon the causes, nature, prevention, and treatment of disease. He has proposed to the Council of the Jenner Institute (lately the British Institute) of Preventive Medicine, that the donation shall be handed over to the Institute on condition that in future the control and management of the affairs of the Institute shall be placed in the hands of a new Board of seven trustees, three of the seven to be chosen by the Council of the Institute, three by the donor, and one by the Council of the Royal Society. The offer has been cordially accepted at a meeting of the Council.

The donor further proposes that part of the new fund shall be appropriated to the enlargement of the buildings of the Institute at Chelsea, part to increasing the at present sadly inadequate salaries of the director and other members of the scientific staff, part to the expenses of administration and maintenance, and the remainder chiefly to founding valuable fellowships and studentships, tenable for limited periods, for research either in the laboratories of the Institute or in the centres of outbreaks of disease, whether at home or abroad.

The executors of the late M. Dobrée of Nantes, the shipbuilder, who left a fortune of 13,000,000 francs, have, in accordance with his wishes, bought a large mansion, with a park of 37 acres, and presented it to the town of Nantes for the foundation of a School of Colonial Horticulture, to endow which a sum of one and a quarter million francs is also bequeathed.

The official organ of the Prussian Ministry of the Interior gives some account of the work accomplished, since its constitution three years ago, by the German Central Committee for the establishment of sanatoria for consumptives under the patronage of the German Empress and the presidency of the Imperial Chancellor, Prince Hohenlohe. The great object of the Central Committee was to establish a sufficient number of sanatoria throughout the German Empire, and in this they have met with much encouragement and success.

The Rev. G. Procter, a retired schoolmaster, has bequeathed £3000 to the University of Aberdeen as a contribution to the erection of an observatory at King's College.

We learn from *Science* that the Bussey Institute, Harvard University, has received from Mr. E. D. Morgan $5000 for the equipment of a pathological laboratory, of which Professor Theobold Smith is the director; also that a new greenhouse, costing $7000, has been given anonymously to the Botanical Garden of the University.

We learn from *Science* that the late Mann S. Valentine bequeathed to the town of Richmond, Virginia, his collections of books, MSS., paintings, and anthropological specimens, with his own house to serve as museum-building. This is to be associated with the educational institutions of the State, to publish literary and scientific papers, and to preserve objects of antiquity.

The Thakoor of Gondal, lately a medical student at Edinburgh University, has spent £30,000 on a college "of the type of Eton," which Lord Sandhurst opened the other day.

The Free Library, Museum, and Technical School of Bootle have started a *Journal* for the use of their visitors. It will serve, in the first instance, as a quarterly supplement to the library catalogues, and should do much to help readers to a right use of the library. We learn from it that the re-arrangement of the birds in the museum, under geographical regions, is now nearly finished, as also is the labelling with popular names. There will also be an arrangement of birds in families, according to Claus's "Text-book of Zoology." The arrangement is described in a threepenny handbook. In connection with this a course of six lectures on birds is being given to school teachers. In the same room as the birds are exhibited the invertebrates.

The University of Pennsylvania has determined to establish an experimental menagerie under the direction of Professor Edward J. Conkling, where

the animals will be permitted to live under the most natural conditions. Experiments will be made to discover how animals communicate with one another, whether they reason, whether domestic animals have increased powers of expression, whether animals, as a class, dream, and so on. Much attention is to be given to the night-flying animals and birds, of which so little is known.

The *Lancet* reports the appearance of a veritable giant in the subalpine town of Cuneo, who is a phenomenon deserving of more than vulgar curiosity. This overgrown individual is a native of the Piedmontese hamlet of Vinadio, where he was born twenty-two years ago. His altitude is (in metres) 2·25, his thoracic circumference is 1·60, and his feet are 45 centimetres long.

"The Resources of the Sea; or, An Inquiry into the Experiments on Trawling and the Closure of Areas," is the title of a work by Prof. McIntosh (C. J. Clay and Sons, Cambridge University Press Warehouse), to be issued shortly. The work gives the results of many years' experience in the department—from Lord Dalhousie's Commission (1883-85) onwards. It commences with a general sketch of the resources of the sea in the various groups, then reviews the Trawling Report of 1884, the present condition of the fishing-grounds and the fisheries, and comments on the changes which have since occurred in vessels and their equipment. The mode in which the trawling experiments have been carried out by the Scotch Fishery Board is then dealt with in connection with the closed areas, and as the author suggested these closures for experiment, his criticisms have some interest. The whole series of the experiments in St. Andrews Bay, the Forth, the Moray Frith, and the Clyde are minutely reviewed and criticised, and, finally, conclusions are drawn in accordance with the facts. These conclusions are adverse to the closure, and show that the influence of man in regard to the destruction of any marine food-fish on an open seaboard is comparatively insignificant. The work is accompanied by thirty-two tables and various photographs and figures.

# Natural Science

A Monthly Review of Scientific Progress

March 1899

## NOTES AND COMMENTS.

### Endowment of Research.

It has been held by many that the scanty endowment of research in Great Britain is the main reason why we fail to produce so large and useful an output of original work as emanates from some other countries. In a few years' time we shall have an opportunity of estimating the justice of this contention in the case of at least one branch of experimental science. Lord Iveagh's princely gift of a quarter of a million sterling to the Jenner Institute of Preventive Medicine should afford a crucial test of what endowment, on a truly munificent scale, can effect. Most of the workers in Preventive Medicine have been, and are, men engaged in professional occupations, and unable to devote themselves exclusively to research. With the funds now at the command of the Jenner Institute, there should be little difficulty in furnishing a capable band of workers with incomes sufficient to free them from the restrictions imposed by ordinary practice. Preventive medicine is a young and rapidly growing branch of science, and there is no subject in which greater advances may be anticipated in the near future. The results which should be brought about by a wise use of Lord Iveagh's gift ought to place the Jenner Institute at least on the scientific level of the Institut Pasteur, and the value of the work done should form so convincing an argument in favour of the endowment of research that other wealthy men would be tempted to follow his generous and public-spirited example. Everything depends on how the money is spent. The Council of the Jenner Institute includes a number of very eminent scientific men whose very names seem to afford a guarantee that its new-found wealth will be wisely and rightly applied. Hitherto the Institute has not been too amply provided with funds, and it has added to its income by a system of lectures and classes in Public Health and Bacteriology similar to those carried on at most of the larger medical schools in London, and for which approximately similar fees were charged. It would appear to us a very unfair thing that any of Lord Iveagh's money should be employed in subsidising this branch of the work of the Jenner Institute to the detriment of existing teaching bodies. We have before us the

Syllabus of the Institute for 1899, printed, doubtless, in ignorance of the financial prosperity that lay before it. We have also before us the scale of charges of the Investigation Department, framed on lines somewhat similar to those of the Clinical Research Association. It will clearly henceforth be beneath the dignity of an institution with an endowment of £250,000, to test the sputum for tubercle bacilli for a remuneration of five shillings, or even to offer a complete chemical and bacteriological report on a sample of water for a similar number of guineas. This may be left to those who have to work for their livelihood. The Jenner Institute has now before it a magnificent and unique opportunity for showing what can be accomplished in scientific results where "money is not so much an object as a comfortable home."

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## Economic Entomology.

"The Report of the Proceedings of the Tenth Annual Meeting of the Association of Economic Entomologists" (1898) contains contributions under the names of many well-known American entomologists.

Two different entomologists write on the San José, or Pernicious Scale, an insect which certainly beats the record for literature on a single form, for legislation directed against the pest, and for money spent in combating its ravages.

In the experiments directed against the San José Scale, the feature of the year, as mentioned in the report before us, has been the use of pure paraffin with effective result against the Scale, and often no consequent, or at least permanent, harm to the tree. While most of the experimenters reported favourably, one or two cases were brought forward where sprayed plants were killed, and, so far as experiment has gone, the position may be summed up thus:—(1) Pure kerosene must always be used with great care; (2) it should not be used against young plants or tender fruit-trees; (3) hardier fruit-trees may be sprayed with pure kerosene, but the trees must not be drenched; the spraying should be carried out while the trees are in a dormant state; the day chosen for the spraying should be a bright one, with weather conditions favouring evaporation; (4) in case of recommendation to spray with pure kerosene, "the individual fruit-grower should be advised to experiment in a small way, and so determine for himself in his own locality, and under the local conditions that exist, whether he can use kerosene to advantage."

The caterpillars of the gipsy-moth (*Porthetria dispar*), as told in the report, continue their work of defoliation in the State of Massachusetts. Less than thirty years ago a few moths of this species, introduced into America by a naturalist for indoor experiment, accidentally escaped. In twelve years' time the descendants of these were

numerous enough to cause a plague. The continued spread and destruction caused by the pest attracted the attention of the State Legislature in 1890, which voted 25,000 dollars for exterminative work. This work continues to be prosecuted diligently, and with the last year's grant of 200,000 dollars and expected aid for the next year or two, the Commission appointed hope to exterminate the pest.

This moth has a bad reputation on the Continent for damage to forests, but in England it is said to be rapidly disappearing, being no longer found in localities where some time ago there was no difficulty in procuring specimens.

Mosquitoes and how to get rid of them forms the subject of another article in the report. Some time ago a paragraph went the round of the newspapers stating, among other things, that permanganate of potash in very dilute solution was fatal to mosquitoes in all stages of their life. One part of permanganate in 1500 of solution was said to render the development of the larvae impossible, while a handful thrown into a ten-acre swamp was said to be sufficient to kill the larvae and keep the swamp clear of mosquitoes for a month, and all this at a cost of only twenty-five cents. Although other statements in the paragraph suggested the unreliability and worthlessness of the whole, Dr. Howard, of the Entomological Department in the United States, who was investigating the mosquito problem at the time, made some experiments with permanganate of potash of various strengths, and found that small amounts had no effect whatever against the pest. As a matter of fact, a waggon-load of permanganate would be required to kill in a ten-acre swamp, and as the water at the end of twenty-four hours is in a condition fit to support mosquito life again, the treatment would require to be repeated every two days. Experiments continue to be made under Dr. Howard's direction, paraffin being allowed to cover the surface of the water in marshy places during the mosquito breeding-season. The botanist, however, has complained on the score that the oil was very destructive to water plants.

An interesting little sermon, entitled "Entomological Ethics," was preached by Mr. T. D. A. Cockerell, who, among other matters, discoursed on the question of private ownership and State property in connection with the materials sent for examination to the various State departments, from the text of St. Paul to the Romans, "What then? Shall we sin because we are not under law, but under grace? God forbid!"

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## Great Auks in Ireland.

The *Irish Naturalist* for January contains two short papers on the occurrence of the Great Auk in the North and South of Ireland, by Messrs. R. J. Ussher and W. J. Knowles.

Mr. Ussher's hunting-grounds were the kitchen-middens of the South of Ireland, previously visited by him. The bones which he discovered on this occasion represented portions of the pectoral and pelvic limbs, and a fragment of a left innominate bone. These were found associated with the remains of "domestic animals and fowls (*sic*) . . . Red-deer, . . . burned stones, and charcoal in layers, and great quantities of shells of edible species (*sic*)." The latter half of the paper contains some extracts from a very sensational article by Lady Blake which appeared in the *Victoria Quarterly* for August 1889. The genuineness of these stories, though no doubt published by Lady Blake in all good faith, is seriously open to question. So far, there seems to be not a tittle of evidence in support of any of the statements therein set down.

Mr. Knowles's finds were made in the North of Ireland—Whitepark Bay. These represent portions of the fore and hind limbs and of the pectoral girdle. "The first remains of Great Auk" from Whitepark Bay were, he tells us, "obtained during a careful excavation of a portion of the black layer,"—information of questionable value, inasmuch as he omits to inform us to what period the "black layer" belongs. Just as, in an earlier part of his paper, he tells us that "There were also associated with these remains flint flakes, cores, hammer-stones, and flint scrapers, together with edible molluscs" (*sic*).

We are curious to know the nature of the "physiological preparations" of the Great Auk, referred to in Mr. Knowles's paper. These two papers are illustrated by some rough and somewhat inaccurate sketches, signed by "M. Knowles."

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## About Yeast.

More than a year ago we were startled by the announcement of the discovery, by Dr. Buchner, that the alcoholic fermentation set up by the yeast-plant was due to an enzyme or ferment which he had succeeded in extracting from it. This meant that a long-cherished belief that the action of yeast could not be disassociated from the living plant must be given up, and the process of fermentation be regarded as simply one of a long series of ferment-actions comparable with that of diastase on starch or pepsin on proteid. However, we were reassured by Prof. Reynolds Green, who, in a paper published in the *Annals of Botany* at the end of 1897, stated that he had been unable to extract an alcohol-producing enzyme from yeast. But, alas! for our equanimity! The latest number of the same journal (vol. xii. p. 491) contains another paper by Prof. Green, who has repeated his experiments with special precautions, and is now driven to the conclusion that actively-fermenting yeast-cells do secrete an alcohol-producing

enzyme which can be extracted by appropriate means, and will set up fermentation in sugar-solutions under conditions which preclude any activity of living plant-cells. The process was a perfectly normal one, accompanied by the diminution of the sugar, the production of carbonic acid gas, and the formation of alcohol. The enzyme is easily decomposed, and therefore rapid manipulation during the process of extraction is necessary. The secretion is shown to be intermittent, occurring only during actual fermentation, and as the enzyme is soon decomposed when the activity ceases, it is not found in the resting state of the plant. Owing to these peculiaritics Prof. Green failed to extract any of the ferment in his former experiments. Dr. Buchner obtained the enzyme by subjecting the yeast to high pressures applied by an hydraulic press, but Prof. Green finds one of five atmospheres quite sufficient.

In the same number of the *Annals* Mr. Wager supplies an interesting addition to our knowledge of the cytology of the yeast-plant. The presence or absence of a nucleus has been matter of considerable dispute. While such a structure has been described by many observers, others have declared the so-called nucleus to be merely a mass of proteid or a vacuole. Mr. Wager has made several contributions to the literature of the subject, speaking for the nucleus, but the one just published seems to put the matter beyond doubt. He describes—and his paper is accompanied by a large series of excellent figures—a structure which he states is invariably present in the cells, and which is a perfectly homogeneous body resembling a nucleolus rather than the nucleus of the ordinary plant-cell. In the earlier stages of fermentation this nucleolus is in close contact with a vacuole which contains a granular chromatin-network, and shows a structure in many cases like the chromatin-network of the nuclei of higher plants. In the later stages of fermentation the chromatin-vacuole may disappear, its place being taken by a granular network or a number of chromatin-granules which may be disseminated through the protoplasm or grouped around the nucleolus. Numerous vacuoles are often found in young cells; these apparently fuse to form the single vacuole already described. In the process of budding, the division of the nuclear apparatus does not show any definite stages of karyokinesis, but is to be regarded as a direct division of the nucleolus into two equal or nearly equal parts, accompanied by division of the chromatin-vacuole or network. In spore formation the chromatin disseminated through the protoplasm becomes more or less completely absorbed into the nucleolus which then divides.

One feature of Mr. Wager's work is the cutting of microtome sections of yeast-cells after imbedding and hardening in paraffin. The absence of karyokinetic stages in the division of the nuclear apparatus will perhaps still leave room for doubt in the minds of some cytologists as to the true nuclear character of the structures described.

## Systematic Position of Phoronidea.

Professor L. Roule has published a short paper (*Comptes rendus ac. sci., Paris,* Oct. 1898) upon the above subject. He has followed the development of *Phoronis sabatieri,* and has been led to suggest one more possible origin for the Vertebrata.

He accepts Mr. Masterman's conclusions with regard to the systematic position of *Phoronis,* in that he considers its nearest affinities to be with the so-called "Bryozoa Pterobranches" (or in other words, *Cephalodiscus* and *Rhabdopleura*) and holds that the Chordata may be directly traced through *Actinotrocha.* He is led to this mainly by accepting the homology of Masterman's so-called "notochords" with the similarly-named organ in the Chordata, confirming this author's description of their origin and histological structure.

On the other hand, he finds in *P. sabatieri,* that the "notochord" is unpaired and ventral, instead of paired and lateral. In the species investigated by Masterman the pleurochords are strictly lateral, though the fact that the "oesophagus" enters the pharynx at its antero-dorsal corner gives them a position apparently ventral to the pharynx. Dr. Roule does not furnish any figures, so that one cannot say whether this fact has any bearing upon his conclusions, and in any case the unpaired condition is a remarkable difference in so closely allied a species.

Still more remarkable is the conclusion to which the author is led by these facts. *Actinotrocha* has a ventral notochord, the vertebrate has a dorsal one; what more natural than to turn the former upside down?

Professor Roule reverts to the idea that *Actinotrocha* is a trochophore (in spite of its five coelomic cavities described by Masterman to which he makes no allusion), and hence he speaks of the "notocorde de ces Trochophores," and the old conclusion is eventually reached that "le Vertebré est un Annélide retourné." The author must not, however, be understood to revert to anything so commonplace as the now well-known morphological somersault of the annelid aspiring to Chordate structure. The *Actinotrocha,* already inverted in the pursuit of progress, turns itself horizontally through 180°. Its mouth then becomes the vertebrate neurenteric canal, and its anal extremity becomes moulded into the head of the Vertebrate. Truly there is a divinity that shapes our ends, invert them how we may! Professor Roule's theory of vertebrate origin will doubtless compare favourably with the various speculations centering round the king-crabs, spiders, leeches, and worms from which we are invited to trace our lineage.

## Prickly-Pears on the War-Path.

THE remarkable productiveness exhibited by certain organisms when artificially introduced into new territory outside the area of their distribution, and where they are freed from those natural checks, under the restraining influence of which their protective armour and weapons of offence have gradually arisen, illustrates in a remarkable manner the intensity of the struggle for existence, and is therefore of perennial interest to others besides those directly affected by it.

It is perhaps not to be wondered at that such species are usually if not always objectionable, and it falls to our lot to call attention to one more instance of the harmful nature of such invasions. In a "Preliminary Study of the Prickly-Pears naturalised in New South Wales," Mr. J. H. Maidan, the Director of the Sydney Botanic Garden, supplies us with detailed descriptions, accompanied by numerous photograms of some six or seven species of *Opuntia*, which, originally introduced into the colony in 1789 in connection with the cochineal industry, have escaped from cultivation, and already cover large areas of fertile land with impenetrable entanglements of thorn. The Colonial Government has been compelled to interfere with an Act for the eradication of the pest, but the expulsion of species which are only amenable to the arguments of fire or poison, and possess a hydra-like vitality, in that every minute fragment gives rise to a new individual, promises to be a task of no small difficulty. It is a relief to know that the Opuntias have some redeeming characters, for every one who knows the plants will agree with Mr. Maidan in recognising their desirability from a horticultural point of view. Some species bear edible fruits; others may be employed to form cattle-proof fences, and certain thornless varieties may even be used as fodder plants.

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## "Terminologic Transgressions."

"WHAT'S in a name"? is no doubt a question which has been repeated by many since the day of which we first have record. It is not the members of the Rosaceae alone which have been supposed to smell as sweet by another name. If there be among our readers any who thus dally carelessly with the sacred instrument of thought, let them now be warned, for a day of reckoning is at hand, and their sins shall surely find them out. Let them be warned, we say, for the Chairman of the "Committee on Neuronymy of the American Neurological Association" is abroad, and who shall stay his hand? Hidden under the modest title of "Some Neural Terms" within the covers of "Biological Lectures delivered at Wood's Holl Laboratory during 1896-97," we have from the pen of Professor Burt G. Wilder a sweeping indictment

of modern anatomists. Huxley, who is known to his fellow-countrymen for some other achievements, is here immortalised as guilty alike of "Direct Pecilonymy" and of "Pecilonymy by Permutation." Others, whose names are as yet concealed, are guilty of "Perissology," of "Magnilogy, which is the same as Anatomic Esotery," of the use of "Polychrestic terms," of indifference to the "Paronymic advantages of Mononyms"; nay, worst of all, of "attempting to check terminologic progress by ridicule." From which and all other "verbifactive sins" may we and our readers be delivered! But to those who, blinded by ignorance, refuse to accept forthwith Mr. Wilder's nomenclature, one other argument may be addressed. Be it known, that it is they and they alone who prevent the coming of that millennium when "*every child of ten* shall have a somewhat extended personal acquaintance with the gross anatomy of the mammalian brain." Those whom this argument fails to convince are beyond hope.

Into the details of Professor Wilder's nomenclature we cannot enter here; no doubt we have said enough to induce our readers to seek it at the fountain-head, but one point we cannot pass unnoticed. It is proposed to avoid the use of proper names in terminology, save where these are recommended by their peculiar euphony. Of such "euphonious" names "Johnny M'Whorter" is given as an example. Deep as is our sympathy with the author's aims, we cannot but feel that there is something invidious in this mention of a single example. Those of us who have a standard of euphony which is different, will find some difficulty in deciding exactly what proper names are legitimate; in determining whether it is the affectionate modification of the first name, the prefix of the second, or the fine full sound of its penultimate syllable, which to the Professor's ear imparts the special charm to the example. A list of suitable names would, we think, be a valuable addition to the next paper.

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## Early Life on Earth.

Captain F. W. Hutton's presidential address to the Geological Section of the Australasian Association for the Advancement of Science has at last reached this country in its printed form. It deals with "Early Life on Earth," and gives a short account of the oldest known fossils. *Eozoon* is rejected from the organic world, while the limestone and graphitic beds of the Grenville series of Canada do not appeal to Captain Hutton as evidence for contemporary life; they may, he thinks, have been due to the decomposition of calcium carbides by hot water. The radiolarian and sponge spicules found by C. Barrois and L. Cayeux in the Archaean rocks of Brittany are regarded as the earliest undoubted traces of life, but necessarily indicate the existence of organised protoplasm at a far earlier period.

There is not much professedly original in the address; but when most people are trying to overcome difficulties by postulating a more rapid rate of variation in the early stages of organic life on earth, it is interesting to find Captain Hutton believing in a slower rate of variation at that period. He bases this Antipodean view on the great thickness of Archaean and early Palaeozoic rocks, and considers that the representation of all the sub-kingdoms of animals in fossils of Cambrian, or at all events of Ordovician age does not imply so great an amount of evolution as do the subsequent developments. "It was this slow rate of variation in ancient times that enabled the early Palaeozoic genera to spread so much more widely over the earth than do the genera of the present day." It must not, however, be forgotten that some would ascribe the great relative thickness of early sediments to greater intensity of denudation on an earth unprotected by vegetation.

## The Passing of the Vanquished.

THE extinction of whole groups of animals, as of the graptolites in the Carboniferous period, and the trilobites in the Permian, has always been a puzzle to naturalists, and on this Captain Hutton has some suggestive remarks. The existence in early times of Radiolaria, almost identical in structure with their descendants of the present day, suggests to him, in opposition to the views of H. S. Williams, "that there is no inherent necessity for organisms to vary or decay, while the idea that if they vary then they must subsequently decay is opposed to the whole teaching of organic evolution, for it is the variable groups which have progressed." The extinction of a whole group must therefore be due to external agencies, and if the group was widely spread, these cannot have been local in their operation.

Change of climate may, perhaps, sometimes account for the extermination of a group of terrestrial animals or plants, but it cannot greatly have affected those living in the sea. "The struggle for existence with other animals has, no doubt, generally been the most efficient cause of extinction, and with pelagic animals it is probably the only cause." The graptolites can hardly have succumbed to want of food, but probably served as food for others. Those others may have been medusae or pelagic cephalopods. Captain Hutton favours the latter, but admits that we know very little about them. The trilobites, on the other hand, were, he thinks, preyed upon by the ground cephalopods, which increased in numbers as the trilobites decreased. In vain the latter acquired the power of rolling up into a ball: "the ruthless intruders turned them over and tore out their insides."

We have unrolled Captain Hutton's pamphlet, turned it over and torn out its inside for our readers. But we hope that the gallant author will not become extinct just yet.

## The Functions of Marine Stations.

WHAT are the functions of a local Marine Station? The question is suggested by the perusal of the "Twelfth Annual Report of the Liverpool Marine Biology Committee," which has just reached us. It contains not only a general account of the work of the Port Erin Station during the past year, but a stirring address by Prof. Herdman on some proposed extensions of its work. Some twelve years ago the Liverpool Committee began the investigation of the marine life of the bay; since that time their sphere of operations has gradually extended, as the members have plunged more and more deeply into the work, and now we have an eloquent protest from Prof. Herdman against artificial geographical limitations, and an appeal for means to carry on work on a more elaborate scale. The problems of distribution, which in their local aspect were one of the prime objects of the Committee's investigation, prove to be insoluble unless attacked on a large scale. The migration of food-fishes, again primarily a local question, is intimately bound up with the plankton-bearing currents of the open seas, and it is in these open seas, and not merely in local waters, that investigation must be carried on. So it is with the other problems which have forced themselves on the notice of the workers at the station. Prof. Herdman therefore appeals, in the first place, for a British Prince of Monaco, whose yacht may enable the Committee to carry out an extensive series of observations in the open sea, and also for funds to improve the station, and carry out adequately the publication of a series of memoirs on common marine animals—another scheme at present in contemplation. We sincerely trust that this appeal to the merchant princes of Liverpool will not be in vain, and that they will hasten to wipe away the reproach of their apathy as contrasted with the liberality with which wealthy Americans so often endow science. We cannot, indeed, but regard it as remarkable that a wealthy city like Liverpool, with all its traditions as a seaport, should be unable to offer its Marine Station more than the very small sum at present at its disposal. If successful and persevering work with small means deserves encouragement, it certainly should not be lacking to the Port Erin Station. We wish Prof. Herdman and his colleagues all success in the carrying out of their enlarged conception of local research.

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## Salt-developed Succulence

HALOPHYTES we have always with us, and also the problem of the causes underlying the succulent xerophytic structure so characteristic of this group of almost aquatic plants. The opinion usually held has been to the effect that the saline character of the water rendered its

absorption by the plant a matter of considerable difficulty, that in short the conditions of life might be described in the well-known words "water, water, everywhere, but not a drop to drink."

Schimper's experiments ("Indo-malayische Strandflora") suggested that salt exercises a poisonous influence on plant life, and he concluded that structural adaptations directed towards the reduction of transpiration were brought about by the necessity of keeping the relative amount of salt in the cell sap below a certain point, which varies with the species.

Stahl (*Bot. Zeit.* 1894) pointed out from observations on artificial cultures that the stomata of Halophytes are completely paralysed, the apertures remaining permanently open, thus compelling the plant to take refuge in other structural modifications in order to limit the transpiration current; hence the succulence.

Professor Diels (*Pringsheim's Jahrb. f. Wiss. Bot.* xxxii. 1898) has been unable to discover in plants growing under natural conditions any stomatic paralysis such as that described by Stahl, and he also differs from Schimper in holding that the concentration of sodium chloride is kept below the danger point, not by limitation of transpiration, but by a chemical decomposition of the salt which at the specific limit of concentration balances the absorption. The process of respiration in succulents differs from that in other plants in that oxidation does not proceed quite so far, but stops at malic acid or some isomer with which the cell sap becomes saturated, while only small quantities of carbonic acid are evolved.

Professor Diels employs this inherent acidity of temper, only obtainable by the development of succulence, in decomposing the excess of sodium chloride, with the result that the plant is enabled to exist unharmed in the bitter waters of its Marah. The chemical process by which the decomposition of the salt is effected is not as yet known, but the author assumes that the malic acid combines with the sodium to form a salt which is of further use in the plant economy, while the hydrochloric acid is excreted by the roots.

## Chalazogams

Since Treub first called attention to the chalazogamy of Casuarines (*Ann. Jard. Bot. Buitenzorg*, 1891) many examples of the non-micropylar growth of the pollen tube have been recorded, and the theory of the evolution of normal porogamic dicotyledons from the chalazogamic type has been steadily upheld by several botanists, among whom perhaps the best known is Professor Nawaschin.

In his most recent paper ("Ueber das Verhalten des Pollenschlauches bei der Ulme," *Bull. Acad. Sci. St. Petersbourg*, 1898)

Professor Nawaschin points out that the condition in the elm is transitional between the lower chalazogamic and the assumedly higher porogamic types.

The ovary of *Ulmus*, in common with that of the typically chalazogamic forms, is devoid of the conducting tissue so characteristic of those cases in which the nutrition of the pollen tube requires to be provided for in its passage across the ovarian cavity.

The ovule is of the pendulous inverted type, and in the great majority of cases the pollen tube reaches it by growing down through the tissue of the funicle not far from the surface till nearly opposite the middle of the ovule when it strikes across through the integuments, the outer of which is but slightly developed, to the apex of the nucellus, thus avoiding alike micropyle and chalaza. In rare cases the tube grows quite close to the surface of the funicle, in fact among the epidermal cells, and even exhibits a tendency to project into the surrounding cavity. These the author cites as indications of a reaching out toward a higher porogamic life. On the other hand, the tube sometimes enters the deeper tissues of the funicle and grows directly towards the chalaza, where however it is stopped by a patch of apparently cuticularised tissue. Here Professor Nawaschin sees a reversion toward the ancestral chalazogamic condition typically represented in Casuarineae, Betulineae, and Juglandeae. The extent to which differences of this kind are to be treated as supplying a basis for classification is at least a question open to discussion.

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## A Theory of Colour Vision.

Believing that the problem of colour vision is "primarily a mechanical one," Dr. W. Patten, writing in the *American Naturalist* for November 1898, has given us a first instalment of an attempt to unravel the mechanism. His brief notice of the Young-Helmholtz theory is introduced by the words, "In Sir Isaac Newton's time there were supposed to be three sets of fibres in the retina!" Dr. Patten's mechanical theory is based upon his claim to have found in the rods of certain eyes—though not in those of the vertebrates—fine nerve fibrils which traverse the rod always at right angles to the direction of the light. As the light travels through the rods these fibrils are stimulated in some way by the ether waves. In cones these fibrils would naturally present regular diminishing scales, the longest at the base, and the shortest at the tip; the longest might, according to Dr. Patten, be stimulated by the longer red waves, the shortest by the violet waves. This is practically the whole theory, which rests upon the gradual variation in the *length* of the transverse fibrils if they could be shown to exist in cones. Passing over this stumbling-block,

that these necessary fibrils have never been discovered in human eyes which we know for certain have a scale of colour sensation, we would remind Dr. Patten that the little evidence which we have—however unsatisfactory we admit it to be—points rather to the tips of the rods or cones as being sensitive to red, and the bases to the violet, which is the reverse of what his theory demands. Nor again do we admit that "every one knows" that the nerve fibres are discontinuous, as taught among others by Ramon y Cajal. It is true that this doctrine has been very hastily accepted, but signs of reaction are not wanting.

One curious part of Dr. Patten's theory is that the optic ganglion, having in some animals (e.g. *Acilius*) a slight resemblance in shape to the retina, and its "Punct-substanz," and being composed of similar fine "vibrating" fibrils, forms with the retina an apparatus suggestive of "a Marconi transmitter and receiver"!

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## Adaptation.

UNDER the title "Zweckmässigkeit und Anpassung," Dr. J. W. Spengel has published, with Gustav Fischer of Jena, a recent academical address. In popular language adapted to a mixed audience, he here discusses one of the vexed problems of evolution. Finding plants and animals fitting perfectly into and reacting with their surroundings, like "ready-made machines dropped down from heaven," we have, alas! no certain answer to the question "how they came there." While admitting that we *must* use the word "adaptation," it is yet, to the author, one of these beautiful words which always pop up "wo Begriffe fehlen," as Goethe's Mephistopheles points out to encourage students of theology. Let us be clear then what we mean. Does "Adaptation" mean "adapted from without" or "adapting itself from within?" In very limited and quite unimportant matters the word may be admitted in both senses. But to show how inadequate these are to explain the facts, he gives a list of cases in which he contends neither would apply. One such case, for example, is the perfect fitness of the whale to its marine life in cold latitudes. In reference then to the essential mechanics of evolution the word adaptation does not mean a process but a fact. His "Anpassung" is therefore nothing more than his "Zweckmässigkeit," and he looks elsewhere for its explanation.

The best answer, he concludes, is that given by the "Natural Selection" of congenital variations. It may be doubted, however, whether Dr. Spengel's arguments against direct modification of structure in response to changes in the environment, and the gradual inheritance of these modifications, attain that degree of cogency which can be called convincing.

## Bibliography of Zoology.

Dr. H. H. Field, writing in the *American Naturalist* for December 1898, states that the work of the Concilium Bibliographicum, hitherto carried on with heavy pecuniary loss to himself, has now been made safe for the future by a permanent subsidy voted to it by the Swiss Confederation, the canton, and the town of Zurich. The office now has its own composing-room, where three type-setters and a head typographer are at work all day. Hard by it has its own large cylinder press, as well as a paper-cutting machine in charge of a special machinist. With this staff 100 cards a day can be turned out, and this is estimated to be more than double the actual rate of zoological publication. A separate staff is employed to sort and check the cards, and to distribute them to subscribers. The use of the classificatory numbers enables this to be done with an almost automatic precision. This system of numbers is a purely practical device for enabling the cards to be sorted at once into their assigned places by any one that can read Arabic numerals. For purposes of subscription almost any conceivable topic may be ordered, no matter how restricted it may be; the price varies from one-tenth of a penny to a halfpenny a card, according to the size of the order. These details refer to the zoological portion of the catalogue only; but the anatomical and physiological bibliographies are also in an advanced state of preparation, and will soon be supplied with regularity.

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## The Museum and Gardens of Trivandrum.

Trivandrum, the capital of Travancore, in the extreme south of India, has a museum and public gardens. The director of these is Mr. Harold S. Ferguson, who recently gave two interesting lectures on these institutions. From the report published in the *Western Star* we learn that both were originated by Mr. J. A. Brown, who in 1852 was appointed astronomer at Trivandrum. This most enthusiastic worker, though strongly supported by the Resident, General Cullen, was unable to make much headway against the peculiar ideas of the native authorities. His successors, the botanist Colonel Heber Drury, Captain Drury, Major Davidson, and finally the chaplain, Mr. Pettigrew, were not more successful, though all, except Captain Drury, had correct ideas of what such a museum should be. In 1879 the management of the museum was placed in the hands of a committee of three, presided over by the British Resident, and, thanks chiefly to the honorary secretary, Colonel Ketchen, the whole establishment was reorganised. Mr. Ferguson himself joined the committee in 1880, and in 1886-87

spent his furlough in studying museums at home. The conclusions to which he then came were in accordance with the enlightened ideas of Sir William Flower, and were fully accepted by the committee. Mr. Ferguson was given full power to put his plans into effect so far as means permitted. In 1890 he succeeded Colonel Ketchen as secretary, and had charge of the Zoological and Public Gardens as well. In 1894 the committee was abolished, and Mr. Ferguson left in sole charge. He appears to have proved worthy of the trust placed in him.

The museum building, completed in 1880, is a beautiful one, though, like many fine buildings, not perfectly adapted to museum requirements. It consists of a main central hall, 70 feet by 40 feet, with walls 35 feet high. Two wings, each 45 feet by 20 feet, with walls 15 feet high, join this to two other rooms 50 by 30, with walls 25 feet high. The main hall is given up to the representation of Travancore arts, manufactures, archaeology, and ethnology. In the south hall are the invertebrates; the wing joining it to the central hall contains the reptiles and amphibians, and will eventually hold the fishes. The north wing is devoted to birds, and the north room to mammals. No space seems to be left for geology and botany, and these subjects, we are told, are but poorly represented. The exhibited systematic series of animals is naturally almost restricted to the fauna of Travancore; but in connection with each of the classes is, or will be, an introductory index series. Such a method of arrangement is, in our opinion, an improvement on the model, namely, the Natural History Museum in London. It permits more ready cross-reference and comparison.

The Zoological and Botanical Gardens, though sanctioned in 1859, were not begun in earnest till 1864. The work progressed steadily, though slowly, in the hands of Mr. Brown, Major Davidson, Mr. Pettigrew, and the committee mentioned above. The grounds generally were brought to their present satisfactory condition by Mr. Ingleby, who came from Kew in 1891 and remained as Superintendent till 1897. In 1867 various animals were transferred from the menagerie of the Maharajah. Better houses were subsequently built for the larger animals, on the basis of plans supplied by the Zoological Society of London. Among the animals now in the gardens, Mr. Ferguson mentions the following: The three kinds of monkeys found in Travancore; two lions, obtained from England; a nervous tiger; a pair of black leopards; a hyaena; various Travancore cats; so-called wild dogs; Himalayan and Indian bears; a great one-horned rhinoceros; a Malay tapir; all the Travancore deer; some Indian antelopes; several species of kangaroo and wallaby, which attracted people in thousands from all parts of the country when they first bred; emeus, cassowaries, an African crowned crane, and many other birds; two Hamadryad snakes; a python, which refused food for a year and ten days when it arrived eight years ago; and a large water

lizard, which is death on rats. Mr. Ferguson's lecture contains some interesting notes on these animals and on local superstitions attaching to them. Travancore is to be congratulated on its gardens and museum, and still more on having so capable a public servant as Mr. Ferguson.

## The Transformations of an Earwig.

In 1881 the late Prof. Westwood described, under the name *Dyscritina longisctosa*, an insect from Ceylon, with the head and body of an ordinary earwig, but with a pair of long jointed cercopods on the last abdominal segment instead of the usual forceps. Entomologists have long suspected that this curious insect would prove to be the immature stage of an earwig; this opinion has now received confirmation by the researches of Mr. E. E. Green. In the latest part of the *Transactions of the Entomological Society*, 1898, pp. 381-390, Mr. Green gives an account of the development which he has traced, and Mr. M. Burr contributes systematic notes on the two species of earwig which were reared from two distinct forms of *Dyscritina*. They are referable to the genus *Diplatys*, Serville.

The most remarkable feature in the development of these insects is the method of transformation of the long jointed cercopods into the forceps. In one of the species examined the cercopods are much longer than the insect's body, in the other somewhat shorter; but in both the basal segment of the cercopod is longer and stouter than the succeeding segments. At the last moult but one, all except the basal segments are completely shed; Mr. Green believes that the insects actually bite them off! Within the truncated cercopods the forceps of the adult can be clearly seen, and these are revealed at the final moult.

It is likely that other tropical earwigs will be found to undergo a similar change, contrasting strongly with our European species, which are hatched with simple tail-appendages already resembling the forceps of the adult. The observations of Mr. Green afford valuable support to the view that the forceps are modified cercopods, and indicate the affinity of earwigs to the Thysanura and Orthoptera, in which orders jointed tail-appendages are so characteristic a feature.

# ORIGINAL COMMUNICATIONS.

## Vegetable Animation.

By John H. Wilson, D.Sc., F.R.S.E.

Most people regard plants as stationary or still objects, exhibiting no movement unless such as is due to the action of the wind or other external agent. The light, quivering aspen leaves respond instantly to the faintest zephyr; the great shoulders of the hurricane are needed to sway the bole and boughs of the gnarled oak. Their oscillation does not startle anybody, because inanimate objects possessing elasticity are similarly affected by the air in motion.

Only the slightest reflection is needed to convince us that plants are seldom either quite stationary or quite still. If they are alive their vital activity will find expression in movement of some kind. A long list of instances could be given of parts and organs of plants exhibiting movement so slow as to be inappreciable to the unaided human vision, and yet quick enough to be recorded by instruments of rude construction. The poet tells us that—

> The sun-flower turns on her god, when he sets,
> The same look which she turn'd when he rose.

This phenomenon the botanist restates when he says that the capitulum of *Helianthus* exhibits heliotropism. A climbing stem swings slowly round in space, in an elliptical orbit of growth, until it touches some suitable support, and it thereupon winds itself in a tight spiral round the object. The glandular tentacle of the leaf of sundew bends, with unerring precision, over the captured prey.

If we desire records of violent spontaneous movement on the part of plants we must either turn to the pseudo-scientific literature of a credulous past or to the ultra-scientific vision of the poet. We find the bold delineation of the Tartarian Lamb, and note that "it has something like four feet, and its body is covered with a kind of down. Travellers report that it will suffer no vegetable to grow within a certain distance of its feat." We do not learn from the traveller's tale why this plant quadruped destroys the vegetation round

its "feat." It is a harmless creature compared with one we read of next:—

> Fierce in dread silence on the blasted heath
> Fell Upas sits, the Hydra-Tree of death.
> Lo! from one root, the envenom'd soil below,
> A thousand vegetative serpents grow;
> In shiny rays the scaly monster spreads
> O'er ten square leagues his far-diverging heads.

Although we must not expect to find anything so very sensational as this in our study of vegetable motility, it is not without its surprises. Who, on first seeing the Sensitive Plant shrink from touch, has not exclaimed, "How very wonderful!" or failed to follow up the ejaculation by the query, "How does this extraordinary movement take place?"

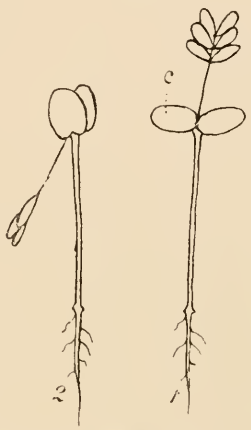
The sensitive plant most commonly grown in greenhouses is *Mimosa pudica;* other species, some of which are only slightly "sensitive," are seldomer seen. The common species is easily grown from seed which can be purchased, or gathered from plants which have flowered indoors. Like many of its congeners it performs the so-called sleep movements, closing up and lowering its leaves at night. During the day the leaves are spread out flat. If, then, a leaflet at the extremity of the divided leaf is touched lightly, in an instant the pair of leaves contiguous to it will flap upwards and meet, then the next pair, and the next, until the whole series approximate. If the stimulus is sufficiently strong, it will pass into the neighbouring secondary leaf-stalks, and not only cause the leaflets they bear to meet, but will induce the leaf-stalks themselves to come together, like the ribs of a fan when being folded up. It is quite possible, with caution, to cause one leaflet alone to move without affecting any of the others. When the stimulus has been severe, the folding up of the leaflets is followed by the drooping of the primary leaf-stalk. A rude shock causes the whole foliage of the plant to assume instantly a collapsed and dead appearance. It is a mistake to suppose that the approximation of the leaflets must take place before the fall of the hinged stalk. By gently pressing on the top of the stalk it can be made to descend without disturbing the leaflets at all. It is noticed that the leaves are more sensitive at a certain stage of their growth than later, and that the maximum degree of irritability is reached at a certain period of the day.

Recovery is gradual, not sudden, the time taken in the process varying with the age of the leaf and the intensity of the light. Elaborate investigations have shown that the motile power is centralised in the swellings at the base of the leaflets and leaf-stalks. The fluids filling the cells of one side of the cushions are suddenly transferred to the opposite side, the equilibrium is disturbed, and the leaflets or leaf-stalks must needs move in the direction of least

resistance. It is very interesting to observe, if one gives close attention to the appearance of the pale spots at the base of the leaflets, that a flush of dull green passes over them at the moment of movement. This is most easily seen by holding the stimulated leaflets back. The same change of colour is observed in the cushions of the leaf-stalks.

It is evident that, whatever the nature of the route along which the force of stimulus is conveyed, there must be few obstacles. If it is the case that cell is connected with cell by delicate threads of protoplasm which pass through the cell-walls, means of uninterrupted passage is afforded when the signal to contract or relax is shot along the tissues of the cushion.

It is not generally known that the cotyledons of *Mimosa pudica* also respond to touch. They originally form almost the whole bulk of the seed, and, in germination, rise as fleshy, oval, green leaves. In a warm temperature they move upward fairly quickly, through a considerable angle, when touched. It is of more than passing interest to find sensitiveness exhibited by embryonic structures, especially when they differ so greatly from the adult ones in form.




Seedlings of *Mimosa pudica*.
FIG. 1.—Undisturbed.
*c*, cotyledon.
FIG. 2.—Touched (nat. size).

While the leaflets of the Sensitive Plant rise to meet each other when irritated, in *Oxalis* (*Biophytum*) *sensitiva* (a plant occasionally grown in greenhouses) they fall. The leaves of this species are pinnate, not trifoliate, as in our native one, wood sorrel.

The purpose served by the movements described is to secure the protection of the foliage. In *Dionaea* and *Aldrovanda*, two of the series of "Insectivorous Plants" studied so minutely by Darwin, the closure of the leaf-blades is to effect the capture respectively of insects and aquatic animals. It may be mentioned as a practical matter that *Aldrovanda* is not easily kept healthy for long, unless in water free from lime.

To many, familiar with the irritability of the Sensitive Plant, the discovery of visible movement in other organs than leaves may not excite much astonishment. If we cautiously examine the flower of any kind of barberry, the columnar pistil will be seen in the centre, with six stamens around it leaning backward, close to the petals and sepals. If the point of a pencil, or the like, be inserted, as if to reach the nectar glands near the base of the petals, there is every likelihood of at least two of the stamens suddenly falling forward against it. After a short interval the stamens gradually fall back and resume their former position, and become ready to repeat the movement when touched. A very few experiments show that the stamen differs from

the leaflet of the Sensitive Plant, in so far that it does not respond unless it is touched in a certain place. It is an interesting test to bring the point of a needle gradually downward, until it reaches the spot in the filament where sensitiveness is located. The petals can be taken away separately, each with a stamen attached, without causing movement. If a stamen, still attached to the petal, is touched at the sensitive point, it curves inwards suddenly, and after recovery will bend again when stimulated. If separated carefully from the petal, laid on a flat surface, and touched, the seat of movement is very clearly demonstrated, the curvature being then seen to take place entirely at the lower part of the filament. The bending is observed to be greater in some species than in others. There is no response to stimuli when the organs are in course of regaining their equilibrium.

The copious supplies of nectar poured out from the glands which lie at the base of the petals attract bees in numbers to the flowers. The instant the proboscis of the bee is inserted, the stamens touched spring forward and cover the insect's head with pollen. In a second he is off to plunder another flower, bearing the fertilising powder with him. A clump of barberry is redolent of honey.

While the stamens in the barberry fall forward, there are some flowers in which they fall backward, when touched. The most easily procured plant showing this is the rock rose (*Helianthemum vulgare*), a low-growing, somewhat woody plant, common on grassy banks and moorland spots. The centre of the yellow flowers is occupied by a tuft of delicate stamens. They stand close together in the undisturbed flower, but when touched at any point they spread out, and the pistil, previously hidden, is then exposed.

Quite a similar mechanism is seen in *Sparmannia africana*, a Cape plant, having no immediate botanical relationship with our rock rose. Occasionally seen in our greenhouses, *Sparmannia* is a shrub of considerable size, with large hairy leaves and trusses of white flowers standing well above the foliage. The individual flowers rise from a pendent to an erect position when about to open. The petals, four in number, are pure white, with a pink spot and "guiding lines" at their base. They, and the four narrow sepals alternating with them, surround the bunch of brightly coloured stamens. The filaments, or stalks, of the outer stamens are bright yellow, and beaded with curious undulate swellings throughout their whole length. The outermost bear no anthers, and terminate in a distinct purple point. The inner stamens, on the other hand, have crimson filaments, with little or no beading, and they carry large pollen-bearing anthers. Transition-forms between the two kinds occur plentifully, the most interesting being such as bear the rudiments of anthers. In the centre is the small superior, spherical ovary, surmounted by the delicate style which is equal in length to the longest stamens. The flowers have a faint odour, suggestive of whin blossom.

When the day is sunny the flowers open well out, so that the

petals are bent back, and the stamens radiate in all directions. The latter are, however, never so far separated naturally but that the slightest irritation will cause them to spread out farther. If a pencil point be applied at the base of a petal, instant divergence of the

*Sparmannia africana.*

FIG. 3.—Undisturbed. *p*, petal; *sp*, sepal. FIG. 4.—Irritated (nat. size).
FIG. 5.—A stamen (enlarged).

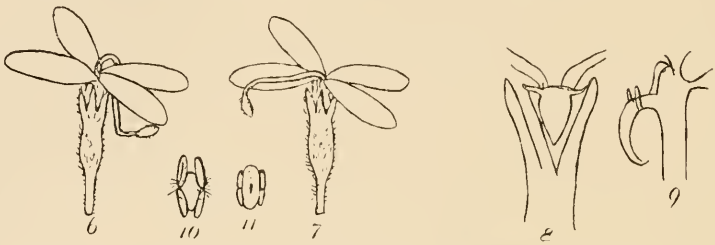
stamens ensues, and the petals and sepals bend still farther back. If the stimulus be severe, the expansion of the tissues uniting the bases of the stamens, and the recurving of the petals and sepals, are so great as to give the flower the appearance of being turned outside in.

The organs return very slowly to the position assumed before stimulation. The swellings on the filaments probably serve the purpose of entangling the head of the insect-visitor, and thus securing more decided irritation. The stigma is well exposed when the maximum spread of the stamens is reached.

The movements exhibited by the androecium of certain composites, notably the corn-flower, have received careful study. If the anthers of a newly-opened floret be touched they are immediately drawn downwards, with a steady motion, by the contraction of the filaments. The pollen, being pressed against the enclosed pistil, wells out for a little at the top of the anther-tube in a steady stream. The stigma is ultimately carried beyond the tube by the elongation of the style.

In the Stylidiaceae, a family very closely related to the Compositae, we find a remarkable modification of the floral mechanism described above. The stamens in *Stylidium* (*Candollea*) are only two in number, and are united with the style to form a trigger-like apparatus, the gynostemium, which hangs outside the flower, and is fixed slightly to a triangular plate—a modified petal—coated with a sticky exudation. Being protandrous, the two anthers occupy at first the summit of this bent column, and in a day or two they wither, and the stigma enlarges

and takes their place. If the action of an insect attempting to enter the flower be imitated, and the lightest touch given in the throat, the protruding organ swings with startling rapidity through an angle of 270°, and comes to lie over the flower with its apex pointing downwards. After a short interval the gynostemium slowly rises and resumes its former position, there to gather energy for another stroke. It is easy to demonstrate that it is most sensitive where it is bent at the throat of the flower. The sudden movement is ostensibly associated with the visits of insects. From the visitor's point of view the reception can hardly be characterised as anything else than violent. One almost feels inclined to think that *Stylidium* has become more annoyed




Flowers and floral parts of *Stylidium*.

FIG. 6.—Flower undisturbed. FIG. 7.—Touched. FIGS. 8, 9.—Sticky shield. FIGS. 10, 11.—Earlier and later conditions of the apex of the gynostemium. (All enlarged.)

than the barberry at the slow pace of the insects, and has acquired means of suddenly stimulating *them* to greater activity.

It may be mentioned that the nettle, and its allies pellitory and the "Artillery Plant" (*Pilea*) spontaneously and violently fling the pollen out of the anthers, once for all, by the sudden release of the tense filaments at maturity.

A movement analogous to that of the leaves of the Sensitive Plant is seen in the closing of the stigmatic lobes of Musk, *Torenia*, *Bignonia*, Butterwort, and certain other plants, when they are touched.

All motility in flowers, whether spontaneous or induced, has reference to fertilisation processes.

We have confined our attention to examples of plants which perform induced movements quick enough to be seen by the unaided eye. We are greatly tempted to regard the phenomena as sentient, and unwittingly fall into the habit of speaking of them as such. An effort is made, by using the rather ambiguous words "irritable," "contractile," and the like, to avoid giving wrong impressions.

ST. ANDREWS.

# The Great Glacial Moraine of Permian Age in South Africa.

By Professor T. Rupert Jones, F.R.S.

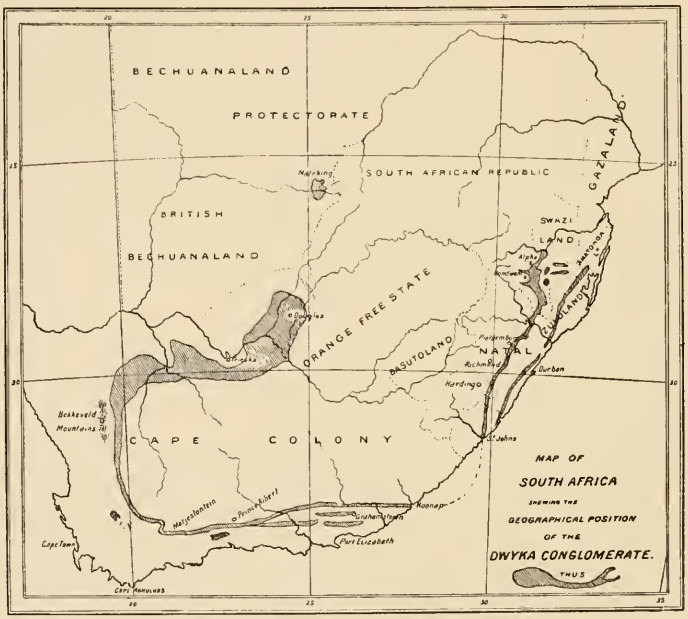
It has often been pointed out that the mountains ranging along and within the coast of the Cape Colony and Zululand, not only have a general parallelism with that coast-line, but consist of parallel groups of stratified formations of schist, sandstone, and conglomerate, with some interruption of granite near the Cape itself, and with a vacant space between Albany and Natal. The upstanding edges of these bedded rocks westward from near the Cape pass in a curved line northward to Namaqualand. On the other side of the Cape they continue into the Eastern Province until they are cut off by the Indian Ocean, south of East London. Nevertheless, as they occur again in the mainland south of, through, and beyond Natal, there has evidently been a loss of the great eastern curve of the parallel ranges, the extreme south-eastern angle of the bluntly triangular southern end of the African continent having been removed by denudation and erosion.

One of the most important of these concentric stratified rocks is the innermost band, known as the "Dwyka Conglomerate," probably of Permian age. It ranges at about a hundred miles or more from the sea in the country north of Cape Town; and after bending eastward at the Cape its distance from the coast is for the most part less than a hundred miles, until it is cut off by the sea at Albany. When it reasserts itself in Natal and Zululand it keeps parallel with, and at no great distance from, the coast; and, as in Albany and elsewhere, is here represented by more than one band.

Mr. A. G. Bain traced this peculiar rock-band from Albany to the Cape (east to west, 450 miles), then on the curve to Toverberg, near the junction of the Doorn and Pataties rivers (50 miles), thence north to Hantam (350 miles). He also noticed two separate narrow masses of this conglomerate to the south of its range, near the Cape, and some parallel duplications, also on the south, in Albany. Thus Bain knew it for about 800 miles in 1856. Since then its northern range on the west side above Hantam and eastward to Douglas (about 50

miles), and northward to Mafeking (about 40 miles), as well as its northward range on the east side, have been mapped by other geologists. From the mouths of the Gualana and Breka rivers to St. John's (40 miles) is a blank, this special band there lying on the ocean-bed. From St. John's to Swaziland is about 70 miles. Altogether, the known length of the outcrop of this "Dwyka Conglomerate" is more than 1000 miles.

Stretching from one side of this southern part of the continent to the other, with a roughly semicircular contour on its southern line, it encloses a large gulf-like area, less oblong and more crescentic than the Bay of Tripoli on the north African coast, and of somewhat greater




Outline Map of South Africa, slightly modified from that published by David Draper in *Trans. Geol. Soc. S. Africa*, 1896, vol. i.

length. Its further extension at the sides to N.W. and N.E., from about lat. 30° S., between the meridians 25° and 33° E. long., probably fails to indicate its real extent.

The locality where this great boulder-band was first specially noticed was at Ecca Vale and Ecca Heights, near Grahamstown; and it has been referred to as the Ecca Conglomerate by some writers, but since E. J. Dunn, formerly Colonial Geologist, noticed a good section of this rock, where traversed by the Dwyka river (one of the headwaters of the Gauritz in Cape Colony), it has been called the "Dwyka Conglomerate."

The constitution of this innermost band of the concentric coast-ranges has been variously described by observers according to its

appearance as seen at different places. It has been described (1) as a rock of igneous origin ("claystone-porphyry"); (2) as being a glacier-formed boulder-clay or moraine; (3) as a breccia composed of angular fragments of volcanic rocks ("trap-breccia"); (4) as having been a long line of beach, consisting of blocks, boulders, pebbles, and sand, with volcanic dust and ashes, on the margin of a wide shallow inland sea or lake, disturbed by subaqueous volcanic eruptions; (5) and lately Dr. G. A. F. Molengraaf, State Geologist, examining this peculiar formation in Zululand, finds evidence that it is really to be regarded as the modified boulder-clay and the moraine of an enormous sheet of land-ice, possibly part of the Antarctic ice-cap of former times; also that, as with other moraines, during the slow retreat of its ice-field, the water draining from it carried away and spread out the pebbles, sand, and mud separated from its coarser materials. These formed special deposits in river-reaches, lakes, and marshes, over a wide area northward of the hollow curve of its line of retreat. This outspread of the morainic material is recognised as the "Ecca Beds," lying parallel with the moraine (Dwyka Conglomerate), all across the Cape Colony and up through Natal and Zululand. Mr. G. W. Stow's "Olive Shales," associated with the "Boulder-drift" of Backhouse and Douglas, north-west of the Hopetown district, come into the same category.

In geological position the Dwyka Conglomerate lies directly upon the Carboniferous quartzite of the Wittebergen and the Zuurbergen, and beneath the Ecca beds, which indeed are interlaced with it at some parts of its range, and pass up into the Karoo beds of Mesozoic age. Hence the glacial conditions, originating the conglomerate or moraine, must have been either in the latest Palaeozoic (Permian) or the earliest Mesozoic times.

How far those ancient glaciers reached northward beyond the present curved morainic border—that is, to what extent glaciers may have occupied the broad hollow, delimited by Palaeozoic rocks, and in which now lie the Ecca and superincumbent Karoo beds, with their concomitant igneous rocks, constituting so large a portion of South Africa—is a subject full of interest.

The long curvature of this ancient moraine may be likened to some of the sinuosities of the irregular line of great moraines crossing North America, and due to the glaciers of the Arctic ice-fields during the relatively modern Glacial Period, often alluded to by geologists as the "Great Ice Age" of Quaternary times.

The mountainous land at the Cape can be only a mere remnant of the high lands once reaching southward to the Arctic Circle, and far beyond it, if continuous with the existing visible polar lands, which are about 30 degrees south of the Cape. (See G. W. Stow's remarks on this continent, *Quart. Journ. Geol. Soc.*, 1871, vol. xxvii. p. 546, etc.)

Whether a perfect polar ice-cap or partial ice-fields originated the

morainic fringe of which the Dwyka Conglomerate remains in evidence, Palaeozoic and Archaean rocks must have pre-existed, and have been covered up and worn down by the glacial covering. And of its scrapings, scorings, and smoothings, evidence doubtless still exists among their hills, valleys, and gorges, their krantzes, kloofs, and poorts in that maritime region, including Namaqualand, Clanwilliam, Malmesbury, Swellendam, George, Kuysa, and their neighbouring divisions, except where obliterated by subsequent weathering.

Some of their morainic materials may have been left as *terminal* like the Dwyka band, or as *local* belts during retreat, like those seen in the Zwarte Rug of the Cold Bokkeveldt, and in the Witteberg, in Cape Colony, and in the Zuurberg in Albany, and near the coast in Zululand.

Some of the memoirs more particularly treating of the Dwyka Conglomerate :—

A. G. Bain, *Trans. Geol. Soc.*, 1845, ser. ii. vol. vii. part ii. p. 54 ; and 1856, part iv. p. 185.
A. Wyley, "Notes on a Journey in Two Directions across the Colony," 1859.
P. C. Sutherland, *Quart. Journ. Geol. Soc.*, 1870, vol. xxvi. p. 514.
C. L. Griesbach, *Quart. Journ. Geol. Soc.*, 1871, vol. xxvii. p. 58.
G. W. Stow, "The Backhouse and Douglas Boulder-drift and Olive Shales," *Quart. Journ. Geol. Soc.*, 1874, vol. xxx. pp. 599, 634, etc.
E. J. Dunn, "Report on the Camdeboo and Neuweldt Coal," 1879.
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# The Penycuik Experiments:[1] An Appreciation.

By J. Arthur Thomson, M.A.

Without raising the vexed question of the relative value of reflection and experiment, or admitting that the antithesis is a just one, we may take it that one of the most hopeful signs of progress in evolution-theory is the increasing prominence of experimental work, and it is on this general ground first of all that we welcome Prof. Cossar Ewart's studies in heredity, well known in interested circles as "The Penycuik Experiments." For it is certain that we have here no fireside musings as to what might be, but the work of a persistent and patient experimenter, with his coat off and his sleeves up, trying to discover what is.

In giving some account of what has been done by Prof. Ewart, one meets the difficulty that most of the points tested are necessarily concerned with individual animals, which, however familiar to the experimenter, are very apt to get mixed in the mind of an outsider. In order, then, to avoid the somewhat fatiguing exercise of keeping Matopo and Mulatto, Romulus and Remus, Biddy and Brenda, Norette and Hekla, as clear individualities in the mind—which is easy enough with the beautifully illustrated book before us—we propose to find the thread of our summary rather in the general ideas than in the fascinating living things. And the ideas round which the experiments have arisen are Reversion, Prepotency, Inbreeding, Telegony.

## *Reversion.*

A great part of the work done concerns zebra hybrids, of which nine (zebrules) were got from the Burchell zebra stallion, Matopo, and various mares, and others from zebra mares served by pony (zebrinnies) and donkey. These hybrids, charming and beautiful creatures, may turn out to be of high practical value, but their present interest is mainly theoretical.

It may be pointed out first of all that the numerous previous

[1] "The Penycuik Experiments," by J. C. Ewart, M.D., F.R.S., Regius Professor of Natural History, University of Edinburgh. 8vo. pp. xciii.+177, with 46 figs. London: A. and C. Black, 1899.

experiments on hybridisation, made by botanists, zoologists, and practical people, have led us to expect one of three results when a crossing has a successful issue. (1) The hybrid may be intermediate between its parents, sometimes so exactly that we may liken the blending to warp and woof; or (2) the hybrid may show an exaggeration of the characters of one parent, often with little apparent realisation of the peculiarities of the other; or (3) the hybrid may be very different from either parent, showing features at first sight novel, but which on closer investigation are sometimes interpretable as the reassertion of the characters of a remoter ancestor. But the extraordinary thing is that at least two of these three different results may be illustrated in one brood or litter.

According to the theory of reversion, confessedly a somewhat unfortunate term, the resemblance which an offspring often exhibits to a more or less distant ancestor, is due to the realisation of characters which were throughout part of the inheritance, but remained latent or unexpressed for one or more generations. As to the fact of resemblance to ancestors there is no more doubt than there is as to resemblance to parents; the theoretical element is simply in the idea of latent characters. If we do not accept the idea that resemblance to ancestors is due to the reassertion of latent elements in the inheritance, we must find some other explanation. And there seem to be two possibilities—(1) that the ancestral resemblance may be due to the fresh and independent occurrence of the same permutations and combinations of germinal material as took place when the ancestral character had its origin; or (2) that the character of resemblance may be an individually acquired "modification," reproduced, apart from inheritance, by a recurrence of suitable external conditions.

It seems impossible to read the literature on the subject without becoming convinced that many phenomena are labelled reversions on the flimsiest of evidence. Thus the occurrence of a Cyclopean human monster with a median eye has been called a reversion to the sea-squirt, and gout has been called a reversion to the reptilian condition of liver and kidneys. Often there has not been the slightest attempt made to discriminate between true reversion (*i.e.* the re-expression of latent ancestral characters) and the phenomena of arrested development, or of abnormalities which have plainly been induced from without. Often, too, there has been no scruple in naming or inventing the ancestor to whom the reversion is supposed to occur, although evidence of the pedigree is awanting; and the vicious circle is not unknown of arguing to the supposed ancestor from the supposed reversion, and then justifying the term reversion from its resemblance to the supposed ancestor. Little allowance has been made for coincidence, and the postulate of characters remaining latent for millions of years is made as glibly as if it were just as simple as a throw-back to a great-grandfather.

Now it is one of the merits of Professor Ewart's work that he has done much to place the doctrine of reversion on a firmer basis of carefully criticised instances. While Mr. Bateson expressed the views of many when he said—"Around the term reversion a singular set of false ideas have gathered themselves," and that "it would probably help the science of biology if the word 'reversion' and the ideas which it denotes were wholly dropped, at all events until variation has been studied much more fully than it has yet been," Professor Ewart's cases will certainly tend to reassure the doubtful as to the reasonableness of the reversion interpretation.

Just as von Baer made the chick pre-eminently the bird of the embryologist, so Darwin made the pigeon pre-eminently the bird of the biologist, and Professor Ewart gives it the first place in his systematised record of experiments. The most striking case is the following:—A pure white fantail cock, which in colour proved to be prepotent over a blue pouter, was mated with a cross previously made between an owl and an archangel, which was far more of an owl than an archangel. The result was a couple of fantail-owl-archangel crosses, one resembling the Shetland rock-pigeon, and the other the blue rock of India. Not only in colour, but in form, attitude, and movements there was an almost complete reversion to the form which is believed to be ancestral to all the domestic pigeons. The only marked difference is a slight arching of the tail. The one parent, a white fantail, belongs to an old-established breed; the other parent, an owl-archangel cross, had already more or less lost the characters of the relatively recent archangel, and had begun to revert towards the blue rock; the progeny of the two was a practically complete reversion. The interpretation suggested is that the older and more stable ancestral units assert themselves successfully in the germinal struggle, while the newer features attain no development.

A few other examples may be noted. An Indian game Dorking cock, crossed with a dark bantam hen, produced amongst others a cockerel almost identical with a jungle fowl (*Gallus bankiva*), *i.e.* with the original wild stock.

A smooth-coated white rabbit, derived from an Angora and a smooth-coated white buck, was mated with a smooth-coated, almost white doe (granddaughter of a Himalaya doe), with very interesting results, significant of the complexity of the conditions. In the litter of three, one is the image of the mother, one is an Angora like the paternal grandmother, and one is a Himalaya like the maternal great-grandmother.

Again, the Burchell zebra-horse hybrids are in their markings very unlike their zebra sire or dam, but bear distinct resemblance in their stripes to the Somaliland zebra (*Equus grevyi*), which the author regards as, in its markings at least, the most primitive of all living zebras. But the evidence from pigeons and rabbits seems stronger than this.

It must be carefully noted that the experiments have been numerous, for it is only thus that the element of coincidence can be duly allowed for. And the general result stands out clearly that the reversion-interpretation has received substantial support.

At the same time it seems extremely doubtful whether such a phenomenon as a complete ulna in a horse, and other cases of the same sort which are cited, furnish any relevant evidence of reversion.

### *Prepotency.*

When an organism of either sex shows great power in transmitting its individual characteristics, it is said to be prepotent as regards these. A stallion or a mare, a bull or a cow, may be so prepotent that its characters reappear in a high percentage of the offspring, no matter what the other parent may be.

It seems doubtful whether anything beyond convenience is gained by the word prepotency—since all these general terms are apt to form the dust-particles of intellectual fog; what we have to do with is the fact that certain variations are markedly stable, heritable, and persistent, almost aggressively persistent one might say. It seems likely that they express positions of relatively great organic equilibrium.

The quality of prepotency is obviously a relative one, and only verifiable in its results. That is to say, it is never more than probable in its exercise. Nor are we able, at the present stage of biological analysis, to define with any precision wherein the secret of prepotency actually lies. We have hardly got beyond imagining that there is a struggle in the germ-cells before and after fertilisation, and that there is a survival of the fitter components within the microcosm of the ovum just as in the macrocosm outside.

Yet, in spite of the obscurity which shrouds the interpretation, the fact of prepotency is certain, and it is of direct human interest not only in connection with the breeding of stock, but also as regards the evolution of races of men. The stud-books show the enormous value of a prepotent sire, and we may regard the persistence of a Celtic, Semitic, or Gipsy strain, in spite of complex intercrossing in the pedigree, as an illustration of prepotency.

This quality, so potent for good or ill, may arise in one of two ways—(*a*) as an attribute of a "sport" or discontinuous variant, or (*b*) as the result of inbreeding. As to the former, Standfuss, who has had so much experience in hybridising butterflies, says that when a sporadic variety is crossed with the normal form of the stock the result is that the offspring agree either with the normal form or with the sport, intermediate forms being absent. ["Handbuch der palaearktischen Gross-Schmetterlinge," 2nd edition. Jena, 1896]. Similarly, Mr. J. W. Tutt reports among the conclusions of Dr. Riding and Mr. Bacot as to hybridising species of *Tephrosia*, that while the (phylo-

genetically) older species is usually dominant, a recently-formed aberration may be prepotent over the type from which it has but recently arisen [*Trans. Entom. Soc. London*, 1898, pp. 17-42]. Mr. Galton holds a similar view, and regards prepotency as itself "a heritable sport or aberrant variation."

While in no way overlooking or combating this position, Prof. Ewart lays emphasis on the second, and probably more frequent origin of prepotency, as the result of inbreeding. "Some breeders say that they can produce a horse so prepotent, so fixed by interbreeding, that it will produce its like however mated"; and there is much evidence to show that, of two parents, the more inbred—up to a certain limit of stability—is likely to have the greater influence on the offspring.

A few examples will suffice. An inbred Dalmatian dog is likely to be prepotent over a collie, a Basset hound over the ancestral bloodhound, an inbred hornless Galloway over one of the long-horned Highland cattle, and Semitic over English blood.

As inbreeding is frequent in nature, especially among gregarious and isolated groups, and as it tends to develop prepotency, we are able to understand better how new variations may have persisted in the course of evolution. The old difficulty that new variations would tend to be swamped or levelled down by intercrossing was met by Romanes and Gulick in their theory of Isolation, some form of which was supposed to limit the range of effective intercrossing. But as this theory has not been sufficiently demonstrated, Prof. Ewart justly insists on the importance of prepotency as an evolutionary factor. But may it not be said that the prepotency which results from inbreeding is itself the result of some form of Isolation? Even preferential mating is a form of Isolation in the wide sense.

Just as in connection with the determination of the sex of the offspring, which may be the resultant of many factors, so in the case of prepotency the result which may be reasonably predicted does not always come off, the natural prepotency being counteracted by other influences due to vigour, age, nutrition, and environment.

The unpredictable nature of the results is well illustrated in Prof. Ewart's observations on crossing wild and tame forms. When white doe-rabbits were paired with wild brown bucks, the progeny resembled the wild form; but of the nine zebra-horse hybrids only two take predominantly after the wild parent. The experiments which others have made on butterflies suggest the conclusion that the older form will tend to be dominant, but this may simply mean that the hybridisation has evoked reversion. We seem to have securer evidence as to prepotency in such a case as the Basset predominant over the bloodhound from which it is derived.

The experiments of Standfuss tended to the conclusion that in hybridising the male parent was prepotent over the female; the young forms were at first liker the female (which may be in part due to the

ovum being relatively much larger than the sperm), but with further growth the resemblance to the male gradually increased. But in the experiments recorded by Mr. Tutt, it is noted that the influence of the male parent seems to be less than that of the female. It seems likely that the discrepancy is to be in part accounted for on the lines of Mr. Vernon's conclusion as to sea-urchins, that the characteristics of the hybrid offspring depend directly on the relative degrees of maturity of the sex-cells of the two parents. Professor Ewart has not found any reason to regard prepotency as correlated with sex. As he cautiously says :—" When allowance is made for reversion, inbreeding, and various other factors, it is extremely difficult to estimate how far the one sex predominates over the other."

*Inbreeding.*

The diversity of opinion in regard to inbreeding, which some exalt as essential to the success of a race, and others decry because of alleged baneful influences, is probably due to the fact that it is beneficial only up to a given limit, and that it is apt to fail prematurely because of some taint in the stock.

On the one hand, it is advantageous in fixing character or developing prepotency, as Professor Ewart illustrates by Dalmatian dogs, Basset hounds, and hornless Galloway cattle. As Galton maintains, the mating of two extraordinary members of two stocks is likely to be followed by a heavy filial regression, while this tends to be slight between two equally-gifted but not extraordinary members of a high-class inbred stock. Moreover, with the extension of an untainted pedigree the risk of serious reversion is probably lessened, for there are probably limits to the duration of latent characters. In other words, the more thorough the inbreeding, within the limits of stability, the less will be the normal quantitative filial regression, and the less will be the risk of the more qualitative reversion. On the other hand, as to disadvantages, there is on *a priori* grounds a necessary lessening of variability, since that is in part conditioned by cross-breeding ; and there is the actual fact that inbreeding often goes too far, and results in loss of vitality and in degeneration. Professor Ewart instances the cases of foxhounds, hogs, guinea-pigs, and race-horses. Sir Everett Millais noted in regard to inbred dogs that distemper carries off about 60 to 70 per cent. of those attacked, and that hereditary deformity and disease tend to be induced.

It may be of interest to recall the experiments which Ritzema Bos made some years ago with rats (*Mus decumanus*). From seven of one family and an unrelated male which died after two crossings, he continued inbreeding for six years, about thirty generations. In 1887 the average number of a litter was $7\frac{1}{2}$, in 1891 $4\frac{7}{12}$, in 1892 $3\frac{1}{5}$. The rate of mortality and the number of infertile pairings greatly increased (*Biol. Centralbl.* 1894, xiv. pp. 75-81).

Believing that the inbreeding of race-horses in Britain has now passed beyond the limit of stability, and that the stock is on the down-grade,—a danger dreaded by many with an anxiety which seems to others uncalled for—Professor Ewart points out that the racer, whose artificial evolution has cost so much, may be saved by bringing in fresh blood in the form of imported Barb and Arab mares (as Sir Everett Millais saved his Bassets by bringing in bloodhounds), or by crossing with imports from America and Australia, which have become somewhat different in their new environment.

All this is of much interest in connection with mankind. Thus it has been maintained, as recently by Reibmayr (*Nat. Sci.* xiv. p. 95), that the evolution of a human race implies alternating periods of dominant inbreeding and dominant cross-breeding. The inbreeding is necessary to give fixity to character, the cross-breeding is necessary to avert degeneracy and to stimulate new variations which form the raw material of future progress. The antithesis between the Jews with their persistent inbreeding, and the complex cross-breeding at present conspicuous in America, is one of almost diagrammatic vividness.

On the vexed question of the sterility of hybrids and what it means when it occurs, Professor Ewart has not as yet been able to shed much light. It is well known that hybrids between different species are sometimes quite fertile, as in the case of the crosses between common goose and Chinese goose, common duck and pintail duck; in other cases, however, the result is sterility. Thus it has not been proved that a female mule has ever produced a foal, though she may produce milk. It is remarkable that the reproductive organs do not seem to have been investigated either in mule or hinny.

In the two-year-old zebra-horse hybrid Romulus, the reproductive organs and instincts seem to be fully developed, but the reproductive elements are still immature (with the merest rudiment of a tail). The same was true of a male zebra-ass hybrid, which unfortunately died. In a female zebra-mule (zebrule), the reproductive organs, which were of a zebroid type, seemed normal, and the ovary showed well-developed follicles; but no proof of fertility has yet been obtained in any case. A nine-year-old zebrinny (horse-zebra hybrid) seemed sterile with both Arab and Clydesdale horses.

### *Telegony.*

The interest which is so often aroused by obscure phenomena is well illustrated in connection with telegony,—or the supposed influence of a sire on offspring not his own, but by the same mother. The literature of the subject suggests that dealing with the somewhat cognate problem of maternal impressions, it tends to be anecdotal rather than precise. The discussion practically dates from Lord Morton's famous letter to the Royal Society (1820), in which he related

that an Arab mare which had borne a hybrid to a quagga, had subsequently colts by a horse, and that these were marked by stripes and by some other peculiarities supposed to be quagga-like. Agassiz, Darwin, Spencer, and others have expressed their belief in the fact; Settegast, Nathusius, Weismann, and others are extremely sceptical; Professor Ewart has followed the path which Romanes had only time to set foot upon, the only secure path, that of definite experiment.

In general terms, he has made a number of experiments likely to give telegony the best possible chance of declaring itself, and although he displays his scientific mood in abstaining from dogmatic conclusion, and in suggesting fifteen other experiments which should be made, the verdict is that so far the evidence of any undoubted telegony is most unsatisfactory. The experiments prove this at least, that telegony does not always occur, indeed that anything suggestive of it occurs only in a very small percentage of cases. Moreover, where peculiar phenomena of inheritance were observed, they seemed to be readily explicable by the reversion hypothesis. It is impossible to withhold admiration when we consider these experiments, involving as they have done so much patience and vigilance, hindered as they have often been by mortality, and very costly withal. If Professor Ewart had originally any bias on the matter, there is no trace of this in his experimentation and exposition.

The most general form of the belief in telegony may be called the infection hypothesis, the idea of which is that the reproductive organs of the mother are specifically infected by having offspring to a particular male,—so specifically infected that her subsequent offspring by other males may exhibit some characteristics of the first sire. The race-horse Blair Athol had a very characteristic blaze, or white bald face; it is said that mares after having foals to Blair Athol, produced Blair Athol-like foals to other stallions utterly unlike Blair Athol. It is supposed that this resemblance was due to an infection of the germinal material by the Blair Athol semen.

A slightly modified form of the infection hypothesis is that of saturation, according to which it is supposed that the characters of the sire expressing themselves in the unborn embryo saturate into the dam, and affect her constitution in such a way that her offspring by subsequent sires inherit some of the characteristics of the first. But while it is conceivable that this may sometimes be the case with a poison or a protective toxin which might diffuse in and out, it seems almost inconceivable when we think of structural characters. We can imagine that a sire infected with some virulent disease, and showing certain structural disturbances associated therewith, may have offspring which are similarly affected, and that the influence from them unborn may saturate into the mother and affect her, so that subsequent offspring by a healthy sire are modified after the manner of the first.

But such cases have a practical rather than a theoretical interest; they hardly touch the problems of heredity or evolution.

On *a priori* grounds, the probabilities are strongly against the occurrence of telegony, but there is no foothold except in the experimental test, and that is what Professor Ewart has given us.

The general nature of the experiments is well known, and for the interesting details the book and its beautiful pictures must be consulted. It may suffice to mention one of the best cases. A Rum

FIG. 1—Matopo.

FIG. 2—Romulus.

pony mare, Mulatto, was served by the Burchell zebra stallion Matopo, and the result was Romulus, with markings quite different from those of his sire, but suggestive rather of the Somali type. In 1897 Mulatto had a bay colt foal to a gray Arab stallion, and this foal—unfortunately short-lived—gave no proof of telegony. The stripes which most frequently occur in horses were absent; there were others which are not uncommon in horses; but the most distinct markings (not that any were strongly developed), namely, those across the croup, were of a sort extremely rare in both foals and horses. In short, the marking of Mulatto's second foal is puzzling, but in no definite

way suggestive of telegony. In this, as in other cases, the verdict must be non-proven.

The psychological problem is interesting, how the belief in telegony has become so very widespread; and the probable answer is twofold, that people are indescribably careless about their beliefs, breeders being notoriously too superstitious, and that the more careful may be easily misled by reversion phenomena which have resulted from the intercrossing. When instances of apparent telegony, of which there are many on record, are carefully analysed, or when the pedigree is traced back, flaws and fallacies are in most cases revealed, as Dr. O.

FIG. 3—Mulatto's Second Foal.

vom Rath has well shown in an intricate family history of cats (*Biol. Centralbl.* 1895, xv. pp. 333, 334).

To sum up: These interesting experiments, of which it is to be hoped we have had only a preliminary instalment, afford vivid illustrations of the unexpectedness of consequences in hybridisation, some cogent scientific evidence of reversion, some analysis of prepotency and its connection with inbreeding, and some demonstration of the difficulty of proving telegony. In more general terms, they have already done not a little towards the fulfilment of Professor Ewart's main intention of making breeding less empirical and the facts of inheritance less obscure; and all biologists will agree in looking with eagerness for more "Penycuik Experiments."

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# The Geographical Distribution of the Arachnida of the Orders Pedipalpi and Solifugae.

By R. I. Pocock.

Since the science of zoo-geography has hitherto been studied mainly from the vertebrate standpoint, one of the most interesting developments of the science in the future will be to discover whether the results obtained by a knowledge of the distribution of the orders of terrestrial invertebrates contradict or coincide with those that have been obtained by a study of the reptiles, birds, and mammals. Moreover, since it is generally admitted that the only means of mapping the various geographical realms, regions, and provinces, on the basis of an acquaintance with the dispersal of all land-species, is the publication on the part of specialists of charts representing the range of the orders, families, and genera with which they alone are familiar, it behoves all systematic workers who are interested in this important branch of zoology to contribute what they can to this desirable result.

In the May number of *Natural Science* for 1894 I briefly discussed the geographical distribution of scorpions, and attempted to map out the regions which the ascertained facts with regard to the range of the families and genera of these Arachnida seemed to establish. In the present paper I propose to deal in the same way with two other orders of the class, namely, the Pedipalpi and Solifugae. Unfortunately, to many of the readers of *Natural Science* these names will perhaps convey no idea of the nature of the animals under discussion; but since it is impossible to enter at length into an explanation of their systematic position, a very few words on this point must suffice.

The term Pedipalpi is applied to a group of Arachnida which, while possessing striking structural peculiarities of its own, lies in many respects midway between scorpions and spiders. It is divisible into two sub-groups; the Uropygi or tail-bearing Pedipalps, in which the last abdominal somite retains, in the form either of a many-jointed feeler or of a one-jointed horny piece, a post-anal sclerite or telson, the homologue of a scorpion's sting; and the Amblypygi or tailless Pedipalps, in which this sclerite has disappeared. These two sub-groups, the Uropygi and Amblypygi, are so very distinct that in the following

pages I have treated them entirely apart. The best known members of the Uropygi are sometimes called whip-scorpions, in allusion to their superficial resemblance to the true scorpions, and to the thread-like form of the long many-jointed tail-piece. They were formerly comprehensively spoken of in works on zoology as *Thelyphonus*. Similarly the Uropygi, which, owing to the shortness and greater width of the body and to the absence of a "tail," are more like spiders than scorpions, are better known collectively as "*Phrynus*."[1]

The Solifugae (or Solpugas) constitute a fourth "group" of Arachnida, very distinct indeed from the scorpions, Pedipalpi, and spiders, though often by the uninitiated confounded with the latter on account of their similarity in external form. The most familiar names connected with this order are *Galeodes* and *Solpuga*.

In many respects these Arachnida and the scorpions are well suited for the study of zoo-geographical problems. In the first place, as compared with other terrestrial invertebrates, they are for the most part of considerable size, and are on that account not likely to have been overlooked by collectors in any of the land-areas, like New Zealand, where they appear to be absent. In the second place, the mutual relationships of the genera[2] are tolerably well understood. In the third place they are practically dependent for their dispersal upon continuity of land, since they cannot fly, and there is no reason to suppose that they are able to swim or even to float; nor is it likely that they can withstand immersion for any length of time in either fresh or salt water, and except in the case of one or two species of scorpions, there is no evidence, nor indeed any reason to think, that their distribution is due to human agency. In the fourth place, since they are exclusively carnivorous, and will eat anything in the way of animal life from small vertebrates to insects, centipedes, and worms, none of the species are limited by the nature of their diet to any one particular locality, their food being universally distributed.

Lastly, although they reach the maximum of their development both as regards size of individuals and numbers of species and genera in the tropical and warmer temperate parts of the world, it would be erroneous to suppose that they are only capable of supporting life in countries where the temperature is warm and fairly uniform throughout the year. Scorpions, for example, are found at high altitudes in the Alps (7000 ft.), and as far to the south in Patagonia as the 50th parallel of latitude, where the mean temperature of winter is a little above freezing-point. Species of *Galeodes* (Solifugae) occur in the steppes of South Russia and Western Asia where the scorching heat of summer is succeeded by a winter of corresponding rigour. Even the whip-scorpions (Thelyphonidae) extend in Eastern Asia up to about

[1] In systematic zoology this name has dropped out of use as a synonym of the older name *Tarantula*.

[2] Except possibly in the case of some genera of Solifugae.

45° N., where, according to Mr. Buchan's charts, the mean temperature of July is the same as that of South Europe, but that of January is lower than that of the north of Scotland. So, too, in the case of the tailless Pedipalpi, although they only pass a short distance to the north of the Tropic of Cancer, yet they have been met with in Patagonia as far to the south as latitude 50°, where the mean annual temperature is only about 40° Fahrenheit, being about 55° in midsummer (January) and 34° in winter (July).

Moreover, the structural differences that obtain between the species capable of withstanding for some months of every year the lowness of temperature expressed or implied in the above statements, and the species that inhabit tropical or, at all events, very much warmer climes, are often only of specific importance. For example, the Manchurian Pedipalp (belonging to the Thelyphonidae) is closely related to one that occurs at Hong Kong, to the south of the northern tropic; the Patagonian tailless Pedipalp is congeneric with a form that is found in equatorial East Africa; the *Galeodes* of the Russian steppes is a near ally of a species that flourishes in the perennial heat of the deserts of South Arabia and Somaliland; and the Alpine scorpion is similarly related to forms that exist in South Italy and Algeria.

So, too, with regard to moisture. Although, broadly speaking, it is true that desert forms are not met with in forest-covered areas, nevertheless genera of Solifugae, which abound in the arid plains of Arabia and North Africa, have representative species in parts of West Africa and India, where vegetation is luxuriant. Congeneric species of Amblypygous Pedipalpi occur in deforested tracts of South Arabia, and in Malabar and Ceylon, where the physical conditions are very different, and a species that has been found in caves in the Philippine Islands has been collected by Mr. Oates beneath stones on the seashore in the Andamans. Nearly allied species of *Butheolus* (scorpions) are met with in Sind, with an annual rainfall of about four inches, and at a place in Satara in the Dekhan, where the average fall is about thirty inches. And lastly, Professor Wood Mason's statement that species of whip-scorpions were only discovered in Assam during the heaviest rains and soon died when removed from their humid surroundings, should be compared with that of M. Schwarz to the effect that the Floridan species frequents dry and sandy spots.

From these data it is evident that the Arachnida in question have very considerable powers of adaptation to varied physical conditions. Consequently their absence from certain areas of the earth's surface must not be too hastily explained away on the plea of unsuitability of environment.

Unfortunately our knowledge of the fossil forms of these animals is extremely scanty. Of the past history of the Solpugas we know absolutely nothing. Pedipalpi, referable both to the Uropygi and Amblypygi, have, however, been discovered in Carboniferous strata.

The Uropygi were represented by the genus *Geralinura*, which apparently differed but little from the existing Thelyphonidae, and has been found both in Europe (Bohemia) and in North America (Illinois). The fossil forms, which seem to me to be referable to the Amblypygi, have been named *Graeophonus* and *Geraphrynus*,[1] the former being based upon remains from the Carboniferous beds of Cape Breton in Nova Scotia, the latter from those of Illinois. But from Palaeozoic times down to our own day the past history of the Pedipalpi is an absolute blank.[2] Nor is our knowledge of fossil scorpions much more complete. Although several genera and species have been described from Upper Silurian and Carboniferous beds, both in North America and Europe, only one species has been recorded as existing during the immense period of time that has elapsed since the Palaeozoic epoch. This was obtained in the oligocene amber beds of the Baltic.

Nevertheless, scanty as are these data, they afford support to the view that these Arachnida originated in, and were widely distributed throughout, the northern hemisphere.

In attempting to establish geographical areas based upon a study of these Arachnids, one important consideration must be borne in mind, namely, that we are dealing with organisms which are wonderfully specialised in particular directions and highly conservative in structural characters. The scorpions, in fact, furnish a famous example of a persistent type of life, and in this particular they do not surpass the Pedipalpi of the family Thelyphonidae. Considering the wonderful changes that have affected the northern hemisphere since Palaeozoic times, it is astonishing that these Arachnids have varied so little. To many zoologists the characters upon which the orders are divided into families, genera, and species may seem of small importance as compared with those of such highly plastic animals as the Mammalia; but keeping in view the fixity of their characters as a whole, it seems clear that what is regarded as a specific or generic difference between two forms may represent a period of separation sufficiently long or a change of environment sufficiently great to produce differences of far higher systematic value in more plastic organisms. Consequently a generic

[1] Described by Mr. Scudder. This is no place to attempt a detailed criticism of this author's work on fossil Arachnida. The expenditure of a little trouble in gaining an acquaintance with the morphology and classification of recent forms would have given his monograph a value to which at present it can lay no claim. Without an examination of the fossils, one's criticisms are, more or less, guess-work, but, judging from the figures, it appears to me that Mr. Scudder has, in more than one instance, mistaken the ventral for the dorsal aspect of his specimens, has referred to the same species specimens presenting characters probably of generic importance, and has placed in the same order, Anthracomarti, representatives of the existing orders of Araneae, Opiliones, and Pedipalpi. The genus *Graeophonus*, which is almost certainly an Amblypygous Pedipalp, he places in the family Geralinuridae, alongside of *Geralinura*, an unmistakable Uropygous form.

[2] The so-called Tertiary *Phrynus* from the Oligocene beds of Aix, cited in works on Palaeontology, and described ten years ago by Gourret as *Phrynus marioni*, is not a *Phrynus* (Pedipalp), as a glance at the figure will show, but a spider allied to the Carboniferous *Arthrolycosa* and the existing *Liphistius*.

difference between the species of two areas may, if desirable, be regarded as of sufficient value to warrant their separation as geographical regions. Similarly the occurrence of the same specific form in two adjacent islands, for example, does not necessarily imply that the separation of the two islands is of recent date.

### *Distribution of the Uropygi.*

The tailed Pedipalps or Uropygi are divided into two sub-orders, the Oxopoei and the Tartarides. The latter is a group that is but little

Map illustrating the geographical distribution of the Uropygous Pedipalps.

known and comprises some very small degenerate forms that have been met with only in Burma, Ceylon, and Venezuela. No doubt they will be discovered elsewhere in the Tropics when adequate search has been made. There is, indeed, a dubious record of a species from Liberia in West Africa; but until the group has been further investigated, it cannot be considered a very important factor in the study of zoo-geography. Two genera are recognised: *Tripeltis*, with a slender cylindrical telson, discovered in Ceylon and Burma, and *Schizonotus*, with an expanded, somewhat spatulate telson, in Ceylon and Venezuela.[1]

The existing forms of Oxopoei are referred to a single family, the Thelyphonidae, divisible into two sub-families, each represented by several genera, which are distributed over the south-eastern

[1] This genus has also been introduced into conservatories in Europe, in connection with exotic plants.

portions of Asia, from Ceylon and China to the Fiji Islands, and in the southern states of North America, Central America, the West Indies, and the tropical parts of South America.

The first sub-family (Thelyphonini) contains the following genera :— *Thelyphonus*, composed of a large number of species, and occurring in Ceylon and South India, Burma, South Siam, and the Philippine Islands; thence over the whole of the Indo-Malayan and the Austro-Malayan Islands to Fiji and the New Hebrides, just touching Cape York, but not extending further into Australia. Very closely allied are the genera *Abalius* and *Tetrabalius*, the former with representative species in New Guinea, New Britain, and Samoa, the latter occurring in Borneo and the Moluccas. *Mimoscorpius*, another allied form, contains a single species recorded from the Philippines. To the north of the area occupied by *Thelyphonus* occur two genera: *Uroproctus*, peculiar to Assam and the eastern parts of Bengal, and *Typopeltis*, which extends from Hong Kong and Formosa into Japan, and a little to the north of the 40th parallel of North latitude in Amurland. In America the sub-family is represented by the single genera *Mastigoproctus*, which has representatives in Arizona, Texas, Florida, Central America, Hayti, Martinique, and in Brazil, as far at all events to the south as Matto Grosso. The second sub-family (Hypoctonini) contains only three genera: *Labochirus* confined to Ceylon and South India; *Hypoctonus* occurring in Silhet, Burma, and Borneo; and *Thelyphonellus*, which has been met with in Guiana and on the Amazons.

This family shows but little structural differentiation. It is consequently difficult to formulate any definite conclusions respecting the geographical regions or sub-regions that may be recognised, on account of the close relationship that obtains between the genera that are found in the eastern and western hemispheres, the differences between them being no greater than those between the species of China and Assam, or Borneo and Java.

For the area in South-Eastern Asia to which these animals are restricted, the term Oriental may be retained, although it is by no means identical with the region of that name defined by Wallace, inasmuch as it embraces part of his Manchurian sub-region of the Palaearctic, as well as the whole of the Austro-Malayan, and part of the Polynesian sub-region of the Australian Region.

The sub-regions of this area are doubtful both in number and extent. The area, however, extending from Corea to Hong-Kong and characterised by the presence of *Typopeltis* and the absence of the more southern types may be recognised as the Manchurian; and if we eliminate as the Assamese the area to which *Uroproctus* is restricted, and as the Ceylonese or Malabar, the part of South India and Ceylon where *Labochirus* is alone found, the rest of the region will correspond very closely to the Malayo-Papuan area so well characterised, as will be explained, by one of the families of Amblypygous Pedipalpi.

In the American continent the area inhabited by this family may be termed the Neotropical. The characteristic genus is *Mastigoproctus*, most nearly allied to the Assamese *Uroproctus*, and extending from the Southern States (California, Texas, and Florida), into the heart of Brazil and including the Greater Antilles. Up to the present time the genus *Thelyphonellus* is only known from the northern parts of South America. On this basis we may perhaps separate the region into two sub-regions, a Northern or Central American, and a Southern or Brazilian.

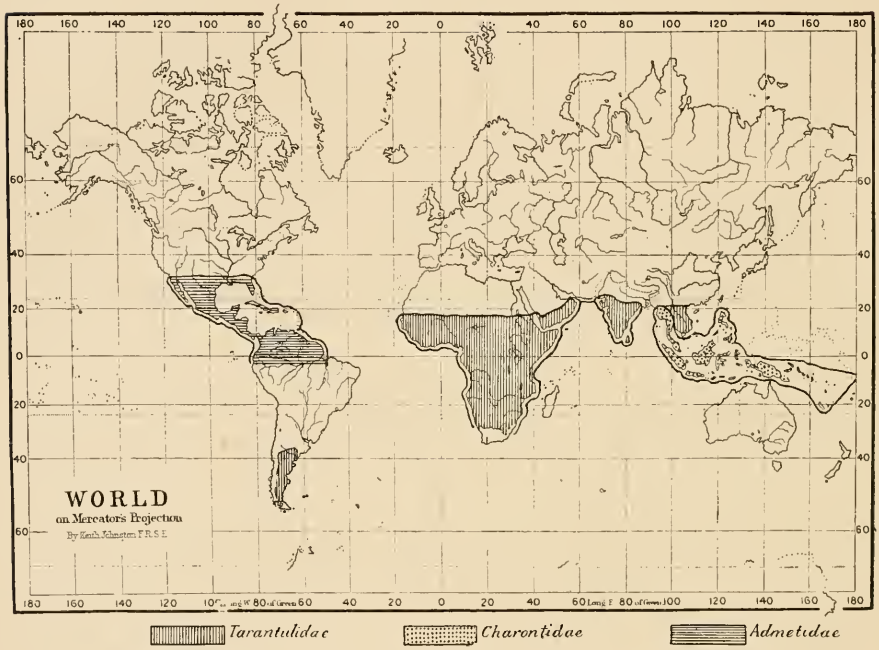
*Distribution of the Amblypygi.*

By certain structural characters which it is beyond the scope of the present paper to discuss, the Tailless Pedipalpi or Amblypygi may be divided into three families — the Admetidae, Charontidae, and Tarantulidae.

The Admetidae are confined to America, where the family is represented by three genera: *Heterophrynus*, with 3 or 4 species, is spread over the whole of the northern part of South America, including Colombia, Venezuela, Guiana, and in the valley of the Amazons as far as Para. Probably the genus spreads into the Brazilian forests, to the south of the Amazons; but there are as yet no data to substantiate this belief. It is at all events absent from the Antilles. This archipelago is peopled by the genus *Admetus* which has been found in Cuba, Hayti, and the Bahamas, southwards to Barbadoes, thence to Trinidad, and over the whole of the area occupied by *Heterophrynus* in South America, and northwards through Central America into Texas and Lower California. The species, however, fall into three well-marked groups, which may perhaps be regarded as of generic value. The first of these, typified by *A. fuscimanus* (*mexicanus*), spreads from Mexico, Cuba, and the Bahamas to Panama; the second (*A. whytei*) is met with in Texas, Lower California, Mexico, and Nicaragua; the third (*A. palmatus*) ranges from Cuba, through the Antilles into Guiana, Colombia, and North Brazil. The third genus which has been established, namely, *Phrynopsis*, is represented by one or two species met with in Mexico and California.

The second family, the Charontidae, ranges from Southern Burma and the Andaman Islands over the whole of the Indo-Malaysian and Austro-Malaysian Islands as far as New Caledonia and Samoa. So far as is known the species are few in number, and each is the representative of a peculiar genus. *Stygophrynus* and *Catagaeus* have been found at Moulmein in Burma. *Charon* extends from Java, Amboina, and the Philippine Islands through Papua to the Solomon Islands. *Sarax* has an equally wide distribution, and has been obtained in the Andaman Islands, Singapore, Borneo, the Philippine Islands, Papua, and New Britain. *Charinus*, the most easterly representative of the family, occurs in New Caledonia, the Fiji Islands, and Samoa.

The third family, the Tarantulidae, has a far wider distribution than either of the preceding two. It spreads over the whole of Africa south of the Sahara, from Senegambia and Abyssinia to Cape Colony, along the south of Arabia from Aden to Muscat, from Bombay southwards into Ceylon and turns up again in Siam, but, so far as is known, does not overlap the range of the Charontidae. Oddly enough, however, the group seems to be represented in South America, according to the independent testimony of two authors. Three genera are recognisable, namely *Titanodamon*, with two or three species, ranging from Senegambia to the Congo in West Africa; *Tarantula*,




Map illustrating the geographical distribution of the Amblypygous Pedipalps.

with about four species, is spread over Eastern Africa from the Mozambique to Abyssinia, in South Arabia, India, Ceylon, and Siam; *Damon* (*Nanodamon*), with two, perhaps more, species occurring throughout East Africa from Cape Colony to Somaliland and South Arabia; in South America, one species has been recorded by Perty from Brazil, another by Simon from Patagonia, a little farther south than the 50th parallel of South latitude, that is to say in about the same latitude as the Falkland Islands.

From these data it appears evident that three geographical regions must be admitted for the Tailless Pedipalps. First the Neotropical, characterised by the Admetidae and one genus of the Tarantulidae, which, however, also occurs in East Africa. This region comprises part at all events of Lower California, of Texas, and probably Florida, the

whole of Central America (with the possible exception of the Mexican plateau), the Bahamas, the West Indies, the tropical parts of South America, and even as far south as Patagonia. This region seems divisible into three sub-regions—the Central American, characterised by the genus *Phrynopsis* and the species of *Admetus* of the *whytei* and *fuscimanus* type, and by the absence of *Heterophrynus* and of the species of *Admetus* of the *palmatus* type. It comprises Central America with part of Lower California and of Texas. Secondly, a Brazilian sub-region containing all the countries of South America to the north of the Amazons, and especially characterised by the presence of *Heterophrynus*. Species of *Admetus* of the *fuscimanus* type invade the western side of this sub-region from Central America, and species of the *palmatus* type, abundant in the West Indies, are also spread throughout the area. Thirdly, an Antillean sub-region seems recognisable owing to the absence of *Heterophrynus* and *Phrynopsis*, the scarcity of species of *Admetus* of the *whytei* and *fuscimanus* types, and the presence of those of the *palmatus* type, which, although also occurring in the Brazilian sub-region, seem to be absent from Central America. To the south of the Brazilian sub-region, a Patagonian sub-region characterised by the absence of Admetidae and the presence of *Damon*, one of the Tarantulidae, may be recognised. This sub-region extends far to the south in Patagonia, but its northern limit is as yet unfixed.

The second region, easily recognisable, may be termed the Malayo-Papuan. It is characterised by the Charontidae, and extends, so far as is known, from Burma in the west to New Caledonia and Samoa in the east, including all the islands of the Indo- and Austro-Malayan areas, but not, so far as has been ascertained, any part of Australia or New Zealand.

And lastly, there is in the Old World the area occupied by the Tarantulidae, comprising Africa south of the Sahara, South Arabia, India, Ceylon, and Siam. For this region the term Indo-African may be applied. The distribution of the genera of this family seems to warrant the recognition of two sub-regions—a western or African, characterised by the presence of the genera *Titanodamon* and *Damon;* and an eastern or Indian, characterised by the almost complete absence of the last-named genera and the presence of the genus *Tarantula*. This sub-region will comprise South Arabia, India, Ceylon, and Siam. The extension of the genus *Tarantula* into East Africa and of *Damon* into Arabia forbids the ascription to these areas of a greater than sub-regional importance.

### *Distribution of the Solifugae.*

The Order Solifugae is divisible into three families:—The Hexisopodidae contains the single genus *Hexisopus* peculiar to South Africa.

The Galeodidae also contains a single genus, *Galeodes*, which has a wide range, extending from the steppes of Southern Russia and Turkestan into Afghanistan, Persia, Baluchistan, India (as far as Madras and Bengal), all over Asia Minor, and even into Greece; also throughout Arabia; and in North Africa, from Egypt to Algeria along the north coast, and southwards through Nubia and Abyssinia into Somaliland. The third family, or Solpugidae, is far richer than the others in numbers, both of genera and species. The following genera are recognised:—*Rhagodes* (*Rhax*), with a range equal in extent to that of *Galeodes* although it is not met with in Greece and Russia, but extends

Map illustrating the distribution of the Solifugae.

in the Saharan region as far as Senegal, and has one reputed species from Ceylon; *Solpuga* (*Zeria*), ranging throughout Africa from Algeria and Egypt to Cape Colony, but attaining its maximum development to the south of the Sahara; *Zeriassa*, nearly allied to the preceding, and occurring in Masailand and Somaliland; *Ceroma*, ranging from Masailand to Cape Colony; *Biton*, found in Syria, Egypt, Tunis, Arabia, and Nubia; *Blossia*, in Lower Egypt and Aden; *Paraleobis*, in Spain, Cape Verde, Somaliland, Socotra; * *Gylippus*, in Syria; * *Gnosippus*, in Egypt; * *Barrus*, in Lower Egypt, and * *Gluvia*, in Portugal; * *Dinorhax*, in Siam and the Moluccas; * *Daesia*, in Mexico; *Datames*, in Colorado, Utah, Nevada, Arizona, Arkansas, Texas, and the Mexican plateau, and

* The genera marked with an asterisk are unknown to me in nature, and I cannot speak with certainty as to their true taxonomic position.

possibly Western Asia (the Caucasus); *Cleobis*, Guatemala, Jamaica, Cuba, Florida, St. Vincent, Colombia; * *Zerbina*, Colombia; *Mummucia*, Peru, Chili, and the Argentine.

In the eastern hemisphere these Arachnida seem to be distributed over three recognisable regions, the Indo-Mediterranean, the Ethiopian, and the Malayan. The Indo-Mediterranean contains Spain, Greece, and South Russia; the whole of North Africa north of a line running from Senegambia on the west to the desert part of Masailand and Somaliland on the east; and in Asia, Asia Minor, Arabia, Persia, Turkestan, Baluchistan, and practically the whole of peninsular India as far to the south as Madras. This region, the richest in genera, is characterised by the occurrence of *Galeodes*, *Rhagodes* (*Rhax*), *Biton*, *Blossia*, *Paraleobis*, *Gylippus*, *Gnosippus*, *Barrus*, and *Gluvia*.

The Ethiopian region, including Africa south of Senegambia and Somaliland, is characterised by the absence of the genera mentioned above and by the presence of the genera *Ceroma* and *Hexisopus*, which are peculiar, and by a host of species of *Solpuga* (including the nearly allied form *Zeriassa*), of which one or two extend into the North African part of the Mediterranean region. No Solifugae have been recorded in West Africa between the Congo and Sierra Leone; but on the eastern side of the continent the Ethiopian fauna blends with that of the Mediterranean area, owing, presumably, to the northern and southern migration of representative genera.

Apart from India, which, possibly with the north of Ceylon, forms part of the Mediterranean region, we know very little, except on the negative side, of the Solifugae of the Oriental region. The only genus that has been recorded is *Dinorhax*, with possibly two species, one in Annam and Cochin China and the other in the Moluccas. No doubt the genus will turn up elsewhere in the islands of Indo- and Austro-Malaya, and probably considerably farther to the north in the Chinese area; and since all the types recorded elsewhere seem to be entirely absent, this area may be termed the Malayan, and be regarded as of regional importance.

The species inhabiting America are distributed so as to admit of the recognition of two regions—a northern or Sonoran, characterised by *Datames*, and probably *Dacsia*, and comprising the Western and Southern States from California to Texas and part of Mexico; and a southern or Neotropical, comprising Central America, with the possible exception of the Mexican plateau, the West Indies, and Florida and the Andean chain in South America as far as Chili and the Argentine, the representative genera being *Cleobis*, *Zerbina*, and *Mummucia*, all of which are apparently closely related.

* The genera marked with an asterisk are unknown to me in nature, and I cannot speak with certainty as to their true taxonomic position.

*General conclusions concerning the Distribution of the Scorpions, Pedipalpi, and Solifugae.*

The available evidence indicates that the Scorpions and Pedipalpi originated in early Palaeozoic times (the Solifugae perhaps later) in the northern hemisphere, and enjoyed a wide range throughout this portion of the globe, migrating from the eastern to the western side, or *vice versâ*, by a land connection across what is now the North Pacific or North Atlantic, possibly by both routes. Their absence at the present day from the whole of the extensive areas that lie to the north of the 40th or 45th parallels of North latitude, is probably to be attributed to the refrigeration of the northern hemisphere during the glacial epoch.

If this view of their northern origin be correct, these Arachnida must have migrated southwards into the southern lands they now inhabit; but since their palaeontology furnishes no help in determining the dates of their migrations, it is necessary to appeal to the evidence supplied by the Vertebrata, and especially the Mammalia, with regard to the past history of the southern continents.[1]

It is considered probable that the ancestors of the existing Afro-Mascarene Mammals fauna entered the Ethiopian region some time during the Oligocene period, when Madagascar was united to South Africa; that during Pliocene times Madagascar was separated from the mainland, and a more intimate union between Africa and Asia, by way of Syria or Arabia, admitted the more highly organised ancestors of the existing ungulates, monkeys, etc., which were thus cut off from Madagascar by the Mozambique channel.

The fact that scorpions belonging to the Buthidae and Ischnuridae, allied to but distinct from the African species, occur in Madagascar, shows that, like the lemurs and the viverrines in the case of the Mammals, representatives of these families inhabited South Africa before the submergence of the land connecting it with Madagascar. Similarly, the absence from this island of the Amblypygous Pedipalpi, of the Solifugae, and of all the African genera of Buthidae and Scorpionidae, points to the conclusion that these forms entered Africa after the separation of Madagascar. Thus, so far as these Arachnida are concerned, the scorpions were the pioneers in taking possession of Africa.

There is no evidence against the hypothesis that these incursions synchronised with those of the Mammalia. On account of the very great antiquity of the scorpions, as compared with the Mammalia, it might be considered probable that the former anticipated the latter in their southward movement into Africa. But, it must be remembered, there is no evidence that scorpions of the kind found in the

[1] In this connection I have specially consulted Mr. Lydekker's "A Geographical History of Mammals. Cambridge, 1896."

Carboniferous beds (Anthracoscorpii) survived that period, and none that the group of Neoscorpii, which comprises all recent forms, had come into existence at that time. Hence it is possible that the latter only date back to the earlier Secondary epoch. They may, in fact, be coeval with the Mammalia. If this be so, the similarity in distribution between the two classes is not a surprising fact.

It is believed by Dr. Blanford that India was connected with South Africa, by way of the Seychelles and Madagascar, throughout Upper Cretaceous (perhaps during Eocene) times, and that the connecting land was broken up into islands at an early date in the Tertiary period. This view is in accordance with the suggestion already made, that the scorpions took possession of the Afro-Mascarene continent during the Oligocene and Miocene epoch, that is to say, after the severance of Madagascar from South India. Otherwise there would be, one may suppose, a much greater resemblance than actually obtains between the scorpions of Indo-Ceylon and Madagascar. Clearly also the absence of Indian scorpions and of Amblypygous and Uropygous Pedipalps from Madagascar, and their presence in South India and Ceylon, shows that they entered these areas of the Oriental region after the submergence of the land uniting this region with the Mascarene portion of the Ethiopian; in other words, not before, and probably after, the Eocene period.

It is stated by Mr. Lydekker that during the Pliocene India and Africa were zoologically identical, the identity being due to the derivation of the mammalian population of the two countries from a common source, namely, the so-called Siwalik fauna of Lower Pliocene age, which has been traced from the frontiers of Baluchistan along the Himalayas into Burma, and for some distance to the north, south, and east of that country. Here, again, there appears to be no evidence against the supposition that the ancestors of the existing Indian, and most of the existing African, Scorpions and Pedipalps, also formed part of the Siwalik fauna. At all events, as a working hypothesis this may be assumed to be the case.

If the Amblypygous Pedipalp of the genus *Tarantula* was widely distributed over the area occupied by the Siwalik fauna, its existence at the present day in Siam, India and Ceylon, Arabia, and East Africa becomes intelligible, because it appears certain that in Pliocene times there was a broad, and in part forest-covered, tract of land uniting Africa with South-Western Asia. Probably the scorpions of the genera *Iomachus* and *Archisometrus*, both of which occur in India and East Africa, entered the latter area at the same time as *Tarantula*.

So far as the genera just mentioned are concerned, the fauna of Africa and India are at the present time identical. The differences between the two that obtain in other respects appear to be explicable on the hypothesis that new forms have sprung up within two areas since the Pliocene epoch, and that others, like the scorpions of the

genera *Scorpiops*, *Chaerilus*, and *Isometrus*, and the Pedipalps of the family Thelyphonidae never succeeded in reaching Africa.

This Pliocene incursion into India took place before the separation of Ceylon. After the occurrence of this event India seems to have been invaded from the north-west, by way of the Punjab and Sind, by its existing genera of Solifugae (*Galeodes* and *Rhagodes* (*Rhax*)), and by the scorpions of the genera *Buthcolus* and *Buthus*. These forms occur in abundance in Persia, Arabia, and neighbouring countries, but are absent from Burma and Ceylon.[1]

The similarity between Indo-Ceylon and the Indo-Malaya, so far as scorpions and Thelyphonidae are concerned, proves either a more intimate union between the two than at present obtains, or a common source in the north, whence a simultaneous southward migration into the two areas took place. Since there is no evidence to be drawn from other sources in favour of the former supposition, the latter may be adopted as furnishing an explanation of the facts. In that case we may conceive that the common source was the Siwalik fauna, which, as has been explained, has left mammalian traces in Burma, China, and various parts of Indo-Malaya (Java, Sumatra, the Philippines).

But whether this be so or not, the extension of the scorpions *Hormurus*, *Isometrus*, and *Archisometrus*, and of Pedipalp *Thelyphonus* right away from India and Burma, and of Amblypygous Pedipalps of the family Charontidae from Burma to Papuasia, or even North-Eastern Australia, shows that there was at that time no barrier to migration between Indo-Malaya and Austro-Malaya. In this respect the Arachnida offer a striking contrast to the Siwalik mammals, which for the most part did not cross "Wallace's line."

Nevertheless from the absence of the Pedipalpi and Solifugae from Australia, it seems evident that this continent, with the exception, perhaps, of its north-eastern portion, was separated from the countries lying to the north of it at the time when the Arachnida in question entered Papuasia. Two of the typically Oriental genera of scorpions, namely, *Isometrus* and *Archisometrus*, are met with in Australia, it is true; but with these exceptions, the scorpions of this area are strikingly different from those of the Oriental region. There is one peculiar genus of Buthidae, *Isometroides;* a second peculiar genus is *Cercophonius*, belonging to the Bothriuridae, a family elsewhere occurring[2] only in South America, where it has many genera and species. A third peculiar genus is *Urodacus*, the type of a special family or sub-family belonging to the same section of the Order as the Scorpionidae of India, Malaysia, and South Africa.

[1] One species of *Rhagodes* has been recorded from Ceylon; but the accuracy of the locality appears to me to be doubtful.

[2] An example of this group has been recorded from Sumatra; but since the specimen appears to be specifically identical with a form common in the Argentine, there can be little doubt that it was not taken in Sumatra. The opportunity of examining this species I owe to the courtesy of M. Simon.

It is impossible to fix the date of the occupation of Australia by the ancestors of these peculiar forms; but if the existing Pedipalp and Scorpion fauna of Indo- and Austro-Malayan spread over this archipelago in the Pliocene period, as has been supposed, it is clear that the ancestors of the Urodacidae must have entered Australia in pre-Pliocene times. Hence it is possible that their entry synchronised with that of the primitive marsupials, and this is believed by Mr Lydekker to have taken place in the Eocene epoch. This author further believes that the similarity between the existing Australian and the extinct Patagonian Dasyuridae (Marsupials) is to be explained on the hypothesis of a land connection in Tertiary times between Australia and South America. This hypothesis would also account for the occurrence in Australia of *Cercophonius*, which belongs to a typically Neotropical family.

In Palaeozoic times it appears that the Scorpion and Pedipalp fauna of North America and of Europe was practically identical, and, judging from the available evidence supplied by the extinct mammals and reptiles, it is permissible to suppose that the similarity extended throughout the Jurassic and Cretaceous epochs. Moreover, during the Tertiary period there must have existed between the eastern and western portion of the northern hemisphere a northern land connection, admitting a free interchange of the more northern representatives of the fauna, at all events during the prevalence of sub-tropical conditions. Nevertheless, even during the time when the northern union was completest and the conditions of temperature most favourable for an interchange of species, we should expect the fauna to become gradually more and more distinct in the eastern and western moieties of the northern hemisphere the farther to the south it extended. These differences would become still more marked with the stoppage of all intercommunication and the destruction of the northern forms with the advent of glacial conditions in late Tertiary times. It seems, in fact, unnecessary to look further afield for an explanation of the generic differences that distinguish the scorpions, whip-scorpions, and Solpugas of the Southern States of North America from the most northern representative of these groups in Europe and Asia.

This leaves the question of the origin of the fauna of South America to be accounted for.

It is held by geologists that South America was separated by sea from the southern portions of North America in the Jurassic and Cretaceous eras, and that the separation, which was probably continued through the Eocene, came to an end by the end of the Miocene. If, then, the ancestors of the existing Scorpions, Pedipalpi, and Solifugae entered South America from North America they must have done so either before the Jurassic epoch or during or after the Miocene. But it appears to me that the structural differences between the South American Arachnida and those of the rest of the world are not of

sufficient importance to warrant the belief that they have been isolated during the enormous length of time that has elapsed since the Jurassic epoch. The species of Thelyphonidae, Solifugae, and most of the scorpions, for example, are only generically different from their old world allies; and, although the scorpions of the family Bothriuridae are, with the exception of the Australian genus *Cercophonius*, peculiar to the region, they are, in my opinion, merely a specialised group descended from the American members of the Vejovidae (Iuridae), a family which at the present time is represented by genera in the Mediterranean and Oriental regions, in California, Mexico, Bolivia, and Chili. It is mainly to the south of the area occupied by the Vejovidae in South America that the Bothriuridae occur in force, and their structure and distribution suggest that they became gradually differentiated as the Vejovidae spread southwards into South America. Fortunately the survival of the Vejovidae has left a clue to the origin of the Bothriuridae.

So, too, in the case of the tailless Pedipalpi of the family Admetidae, which are confined to the region. The structural peculiarities of the genera seem hardly important enough to justify the view that they have been isolated since the early portion of the Secondary epoch; and the supposition that the eastern and western representatives of the Amblypygi were probably cut off from intercommunication with each other at an earlier date than were the scorpions, whip-scorpions, and Solifugae, is supported by the fact that at the present time they do not extend to the north of the Tropic of Cancer in the Old World, and scarcely surpass it in the New. This would lead to an earlier differentiation of the genera on the two sides of the Pacific, and the differences would go on increasing during the gradual refrigeration of the northern hemisphere, which culminated in the glacial period, when the Pedipalps in question were probably exterminated as far south as a line represented by the northern tropic, and the northern ancestors of the Admetidae were blotted out.

If, however, the view that the Neotropical scorpions, Pedipalpi, etc., did not enter South America in pre-Jurassic times be rejected, it still remains to be seen whether any other country than North America can be looked upon as the source that has supplied South America with its fauna.

On palaeontological and geological grounds it is believed by Mr. Lydekker that the ancestors of the mammalian ungulate fauna of South America migrated from South Africa by means of a land connection in Tertiary times, a small contingent of marsupials crossing in the same way and during the same epoch from Australia. This migration of African and Australian forms took place before the northern irruption of the higher mammals, such as cats, bears, llamas, etc., into South America in post-Miocene times; and since, as is believed, the ancestors of the existing population of African eutherian mammals and of Australian

marsupials entered their respective areas of distribution in Eocene times, the lands connecting these two continents with South America must, according to the hypothesis, have been in existence some time between the Eocene and the end of the Miocene periods.

Apart, however, from dates, since land connections between the areas in question are believed to have existed, it is necessary, when discussing the question of the origin of the Neotropical scorpion and Pedipalp fauna, to examine the evidence for or against the view that any elements of the fauna have been derived from Australia or South Africa.

Since the Solifugae and the two groups of Pedipalpi with which we are dealing do not occur in Australia at the present time, and there is no reason to suppose they have existed there in the past, the question of the Australian origin of the Neotropical members of these orders need not be further discussed. But seeing that both Amblypygous and Uropygous Pedipalps extend in Polynesia as far as Samoa, it may be deemed possible that they passed into South America by a land connection in more northern latitudes. No justification for this hypothesis, however, is supplied by the existing fauna.

With regard to the scorpions, the question of a trans-Pacific migration bears a different aspect. As already stated, the Australian genus *Cercophonius* belongs to the typically South American family Bothriuridae (Telegonidae). Hence Australia may have supplied South America with this portion of its fauna. But, *ceteris paribus*, the migration may equally well have taken place the other way, that is to say, from South America to Australia, and this view of the matter is supported by the richness of the Neotropical and the poverty of the Australian fauna in genera and species of this family. If this supposition be correct, and if the assumption already made, that the ancestors of the Neotropical Bothriuridae did not enter South America before the end of the Miocene be also correct, there must have been a land connection between the two countries, probably in Pliocene times. It seems evident, however, that New Zealand formed no part of this trans-oceanic continent.

Turning now to Africa, it is clear that since the Thelyphonidae do not exist in that country, the Neotropical genera of the group cannot have come from there. The Neotropical Solifugae, too, are in no way closely related to the Ethiopian members of this Order, but to those of the Mediterranean region, and no special relationship is traceable between the African Amblypygous Pedipalpi of the family Tarantulidae and the Neotropical Admetidae. The scorpions, too, of the two regions are, on the whole, very distinct. There is, however, one genus of scorpions, *Opisthacanthus*, which at the present time is found only in tropical Africa, Madagascar, and South America, and one genus, *Damon*, of the family Tarantulidae, which occurs only in East Africa and South America.

The occurrence of the genus *Opisthacanthus* in Madagascar as well as in South Africa, indicates that it entered Madagascar before the

severance from Africa was effected, and, according to our hypothesis, this migration took place in Oligocene times. Hence, if a land connection between South Africa and South America existed at any time during or after the Oligocene, scorpions of this genus might have crossed from one continent to the other. That Africa and not South America was the original home of the genus is shown by the existence of many species in the former country and of many allied genera in the same and other areas of the Old World, while the single species that South America possesses is the only representative of the family found in the New World.

The same line of argument might be adopted to explain the occurrence of the Pedipalp *Damon* in the Ethiopian and Neotropical regions, were it not that the genus is not known to inhabit Madagascar. Possibly it may in the future be found in this island, but if not, its absence, as already shown, will indicate that it entered or became evolved in South Africa after the formation of the Mozambique channel; and since this event is believed to have taken place about the Plicoene period, and assuming the present distribution of *Damon* in South America and South Africa to be due to the existence of a southern land connection between the two continents it inhabits, it seems clear that the connecting bridge had not subsided, at all events, before the beginning of the Pliocene.

The available evidence then seems to show that the Neotropical region owes the bulk of its fauna to North America, and that certain elements in it may have come from South Africa. Again, the evidence appears to me to be in favour of the hypothesis that it was peopled with the ancestors of its existing species of scorpions, Pedipalps, and Solpugas during the Tertiary epoch.

The West Indies seem to have acquired their fauna from two sources. The Solifugae and Thelyphonidae probably came from Central America *viâ* Cuba; and a land connection between this island and Yucatan is believed to have existed in the Pliocene and probably in the Plistocene as well. Some of the scorpions and the species of *Admetus* of the *fuscimanus* type appear to have travelled by the same route; while other scorpions and the species of *Admetus* of the *palmatus* type seem to have entered the sub-region from the south *viâ* Venezuela or Guiana.

In a paper upon the geographical distribution of scorpions already referred to, I pointed out that the area of the earth's surface to which these animals are now restricted might be divided into the following regions:—

1. MEDITERRANEAN, including South Europe, North Africa from Senegambia to Nubia, Arabia, Asia Minor, Persia, Afghanistan, etc., and North-Eastern China.
2. ETHIOPIAN, including Africa south of Senegambia and Nubia, and Madagascar.

3. Oriental, including India, Ceylon, Burma, and the countries and islands east and south-east of this point as far as "Wallace's line."
4. Australian, including the Australasian islands east of "Wallace's line" and Australia.
5. Sonoran, including the Southern States of North America south of about the 40th parallel and the central plateau of Mexico.
6. Neotropical, including the rest of Central America, the West Indies and South America, nearly as far south as the 50th parallel of latitude.

Subsequent studies in the same group have not materially affected these conclusions. But the discovery that species of typically Arabian and Persian types extend as far as Gwalior in India will make it necessary to draw the line of demarcation between the Mediterranean and Oriental regions considerably to the east of the Indian frontier.

The known facts connected with the distribution of the Solifugae and the Pedipalpi agree in the main with those that the scorpions supply. A notable exception is furnished by India, which, while clearly belonging to the Oriental region so far as scorpions and whip-scorpions (Uropygi) are concerned, forms part of the Mediterranean region in the case of the Solifugae, and of the Ethiopian in the case of the Amblypygi. Taking the four groups into consideration, however, the balance of evidence is in favour of classifying it with the Oriental region. On the other hand, the Austro-Malayan Islands, which in the case of the scorpions were referred to the Australian region, belong, in the case of both Amblypygous and Uropygous Pedipalpi, and also in the case of the Solifugae, if the record of *Dinorhax* from the Moluccas be correct, to the Oriental region without doubt; and since, even in the case of the scorpions, these islands are perhaps as much Oriental as Australian, the evidence points to their inclusion in the former region.

One last word about the Sonoran region. The absence of any barrier between this region and the Neotropical makes its exact southern limit a matter of doubt, on account of the intermixture of the faunas of the two areas. The genera that may be regarded as characteristic of the region are *Hadrurus*, *Vejovis*, *Uroctonus*, and *Anuroctonus* amongst the scorpions, and *Datames* amongst the Solifugae. These forms extend into Mexico, but are absent from the West Indies. But the region also contains species of Uropygous and Amblypygous Pedipalpi, scorpions of the genera *Diplocentrus* and *Centrurus*, and probably Solifugae of the genus *Cleobis*. Since, however, these genera extend far into the Neotropical region, and are met with in the West Indies, they must be regarded as Neotropical elements, which have pushed their way to the north since the final union between North and South America took place.

# Protoplasmic Currents and Vital Force.

By Prof. A. L. Herrera.

I have lately stated that some currents of granules may lead to the formation of a pseudopodium in my synthetic protoplasm observed under the microscope.[1] What occurs is an exact imitation of the natural phenomenon. The internal energy of the said currents expends itself in external movements. The fluid loaded with granulations strikes, as it were, a blow as it dashes against the endosarc, or the limiting membrane of the protoplasm, and pushes it outwards.

But these currents play a more important part; they induce, indeed, the following processes:—

1st. Renovation of the surfaces of contact between the oxidisable parts and the external oxygen. More effective elimination of carbon dioxide.[2]

2nd. Conveyance of the nutritive particles and residues. Nutrition of the masses of alveolar protoplasm, which fulfil the functions of glands, etc., according to principles of Van't Hoff, Becquerel,[3] and Loeb. Circulation of the reserves and circulation in the zymoses.

3rd. Deposition of certain materials and separation of some others according to their solubility, density, and so forth. Concentric formations, incrustations, etc.

The study of these internal currents is, one may say, the chief aim of physiology. They may be explained in terms of known physico-chemical causes rather than by an undiscovered and undiscoverable vital force. The causes are—

A. Diffusion and osmotic currents.

B. Heat. Oxidations.

C. Ingestion of the materials that support the phenomena of diffusion and oxidation.

D. Partial vacua and changes of every kind in internal pressure, induced by evaporation, etc.

[1] *Natural Science*, August 1898; *Bull. Soc. Zool. France*, 1898, p. 119; *American Naturalist*, December 1898.

[2] See A. L. Herrera and D. Vergara Lope, "New Theory of Respiration." Congress at Moscow, 1898.

[3] Becquerel, "Les forces électro-capillaires dans les phénomènes de nutrition" *Comptes rendus Acad. Sci. Paris*, 15 février 1875.

The action of these causes may be tested by both the natural and the synthetic protoplasm.

A. The use of gummy water is indispensable if one wishes to observe the circulation of protoplasm in the elements of trees, and the movements are generally dependent on the conditions of diffusion (cf. Bütschli's foams).[1] The currents of the artificial product vary in accordance with the diffusive power of the substances, the quantity of liquid, and the presence of some large granulations.

B. The rapidity of diffusion increases, within certain limits, with an elevation of temperature (Graham). The movements of the protoplasm increase in rapidity between 10 and 22 degrees, becoming slower beyond those limits, and stopping between 45 and 48 degrees.

I have seen that at a suitably high temperature these currents present themselves even in very viscous liquids. It is evident that oxygen as well as the liberation of heat attendant on respiration are equally necessary to every being.

C. The paralysis of artificial currents ceases completely with an addition of peptone or a new quantity of salts.

D. This is an evident principle. It is enough to remember the facts concerning the circulation of sap and blood. The paralysis of internal currents stops life everywhere, decomposition coinciding with an absolute diminution of movement.

The rapidity of the course of blood through the capillaries is identical with that of the currents of protoplasm, and varies likewise according to conditions, its result being the same—nutrition and life.

A motionless peripheral layer of serum is observed similar to that apparent in the currents of pseudopodia.

The difference between latent and oscillating life lies, in short, in the almost absolute or simply partial inhibition of the internal currents. Water, heat, and oxygen are required as in a physico-chemical phenomenon, and I have often suspended the currents in my protoplasm by means of desiccation or refrigeration for months together. There is then another argument against my theory which regarded movements as a result of the discharges of carbon dioxide—a theory which has certainly been for me a source of fertile suggestion, though I have now given it up.

The importance of a large quantity of water in internal currents is perfectly demonstrated. I have shown that dilution has a great influence on the rapidity of the granulations in my artificial protoplasm.

Now, the gray substance contains more water than the substance in the cerebellum, and this has more than the white substance of the brain and medulla (R. Dubois). The neuroplasm has doubtless its currents, and the variations exhibited in their rapidity, as well as the shocks of their molecules and the waves produced, perchance, by the passage of the current from a conductor with a big calibre to a thinner

[1] See Milne-Edwards, "Anatomie et physiologie comparée," tome v. p. 105.

one, may result in certain nervous and continuous actions or sensations, external stimuli provoking the vibrations, as I have studied in mercury.[1] On the other hand, Dubois says that anaesthetics produce the expulsion of internal water, and I have observed that exhalations of ether have the property of energetically repelling any thin layers of water ("On a Property of Ether," *Memorias y Revista Sociedad Alzate*, 1895-96, Nos. 5, 6, p. 33). This means that anaesthetics modify the rapidity of the currents or even succeed in completely preventing them.

The action of alcohol on my artificial product is curious, there being a remarkable excitation of the movements followed by their absolute paralysis.

In the sea-urchin egg, says Dubois, segmentation can be prevented by hindering hydration by the addition of salt at 2 per cent. to the sea water. When segmentation has already begun it stops in a strongly salted medium, but it pursues its course directly after some normal water is poured on it; and, what appears more notable, it then continues with increased rapidity. I have observed analogous phenomena in my artificial protoplasm.

In a word, the protoplasmic currents have a constructive or formative action comparable to that wrought by rivers on the earth's surface.

Contractile vacuoles can be explained by an augmentation of tension promoted by some endosmotic currents. The former may be imitated by alternatively stretching and relaxing a plate of gluten.

Life ought not to be likened to a continuous chemical reaction, the mechanism of which remains involved in darkness and unexplained. Life is now to be defined as the result of the physico-chemical action of protoplasmic currents, the cause of such currents being diffusion, heat, and some other secondary factors. Death consists in an absolute suspension of the internal currents in general; latent life is characterised by the establishment of the said currents under the influence of oxygen, heat, and water, in a germ or organism having the structure and chemical elements necessary, and supplied with every nutriment required. Oscillating life is nothing more than an alternate contribution and reassertion of the constructive internal currents, depending upon the variations of the external temperature. Every physico-chemical or mechanical action capable of affecting the rapidity, direction, and other characters of internal currents must have more or less influence on the phenomena hitherto considered as vital.

MEXICO, *January* 9, 1899.

[It may be recalled that the artificial protoplasm to which the author so often refers is an emulsion of albuminoid, etc., made according to Reinke's analysis of "Flowers of Tan." In expounding his conclusions Professor Herrera is at a disadvantage in writing in a foreign tongue; and we may also note that he is also hindered in his researches by the lack of an adequately powerful microscope.—ED. *Nat. Sci.*].

[1] *Natural Science*, December 1898.

# FRESH FACTS.

The Shell of a Turtle. H. Gadow. "Orthogenetic Variation in the Shells of Chelonia," *Proc. Cambridge Phil. Soc.* x. 1899, pp. 35-37. The normal shield of the Loggerhead turtle (*Thalassochelys caretta*) possesses six median (including the so-called nuchal) and five pairs of costal scutes. Of a total of 56 specimens not less than 43 were abnormal, *i.e.* 76·6 per cent. Of 41 newly-hatched specimens not less than 38 were abnormal, *i.e.* 92·7 per cent. The variations are manifold, the number of median scutes varying from 8 to 7 to 6, the costal scutes ranging from 7 to 6 to 5, and they are either symmetrical or uneven. Most of the individuals seem to grow out of these irregularities, which the author regards as "atavistic reminiscences," and the reduction or squeezing-out of the supernumerary scutes proceeds in a very regular way, suggestive of "orthogenesis," it seems.

Poison of Centipedes. O. Duboscq. "Sur l'histogenèse du venin de la scolopendre," *Arch. zool. expér.* vi. 1898 (Notes et Revue) pp. xlix-li. Part of the poison is formed in the nuclei of the glandular cells at the expense of the chromatin, a histological result which agrees with the chemical one that certain active components of poisons are nucleo-albumins.

Secretion of Poison in the Adder. W. Lindemann. "Ueber die Secretionserscheinungen der Giftdrüse der Kreuzotter," *Archiv mikr. Anat.* liii. 1898, pp. 313-321, 1 pl. The process of venom-secretion is closely analogous to that in an ordinary salivary gland. Homogeneous drops appear in the cell-substance which becomes clearer. The periphery becomes darker as the drops are discharged.

Castrating Caterpillars. J. Th. Oudemans. "Falter aus castrirten Raupen, wie sie aussehen und wie sie benehmen," *Zool. Jahrb.* xii. 1898, pp. 71-88, 3 pls. 2 figs. The experimenter has not only shown that it is possible to castrate caterpillars, which is surprising enough, but that the process has no effect on the external appearance of the adults as regards secondary sex characters, which is even more surprising. Even the habits were little affected, thus copulation occurred though there were no spermatozoa. The organism is indeed a unity, and more than a correlated congeries of parts!

Inoculation for Tick Fever. C. J. Pound. "Note on Tick Fever in Cattle," *Journ. Quekett Micr. Soc.* vii. 1898, pp. 118, 119. The author claims to have worked out protective inoculation for tick fever. Some thousands of cattle have been inoculated, and the results have proved highly satisfactory, for when such cattle were subjected to gross tick infection, or injected with virulent blood, they remained perfectly immune. But no statistics are given in the brief paper.

Hairs of Monotremes. Baldwin Spencer and Georgina Sweet. "The Structure and Development of the Hairs of Monotremes and Marsupials, Part I.

Monotremes," *Quart. Journ. Micr. Sci.* xli. 1899, pp. 549-588, 3 pls. and 6 figs. In all essential respects the development of the hairs in Monotremes is precisely similar to that of other mammals; the early development of the follicle is in the form of a solid epidermic downgrowth, not tubular to start with as Poulton described it.

A PARASITE OF THE BRAIN. ALBERT W. BROWN. "On *Tetracotyle petromyzontis*, a Parasite of the Brain of Ammocoetes," *Quart. Journ. Micr. Sci.* xli. 1899, pp. 489-498, 1 pl. The existence of so highly organised a form as a Trematode in the brain cavity of a vertebrate is a unique phenomenon in many ways, and this must commend *Tetracotyle petromyzontis* to the interest of naturalists. It is, the author says, almost incredible that any vertebrate could live on, apparently without discomfort, whilst its brain is packed with hundreds of flukes. But this is the case.

MORE EXPERIMENTAL EMBRYOLOGY. HENRY E. CRAMPTON, JR. "The Ascidian Half-Embyro," *Ann. New York Acad. Sci.* x. 1898, pp. 50-57, 2 pls. An isolated blastomere of *Molgula manhattensis* segments as if still forming a corresponding part of an entire embryo. The cleavage phenomena are strictly partial; the result is a larva of less than normal size, and with defects in certain of its systems. Mr. Crampton's results confirm the view of Roux, that the development begins as a partial one, but that the missing part is gradually supplied by the cells already present, so that the partial nature of the development is progressively masked, and the end is a nearly complete larva.

INTERCELLULAR BRIDGES. HERMANN KLAATSCH. "Die Intercellularstructuren an der Keimblase des Amphioxus," *SB. Akad. Wiss. Berlin*, 1898, pp. 800-806, 4 figs. There is a well-developed system of intercellular connections persistent to the end of the gastrulation process, if not longer. Ectodermic cells are connected, and endodermic cells are connected, but there is no connection between ectoderm and endoderm except at the transitional zone.

NEW CASE OF PARENTAL CARE IN FROGS. AUGUST BRAUER. "Ein neuer Fall von Brutpflege bei Fröschen," *Zool. Jahrb.* xii. 1898, pp. 89-94, 3 figs. Dr. Brauer found in the Seychelles a small frog (*Arthroleptis seychellensis*) which bore on its (his ?) back nine tadpoles fixed by their ventral surface.

TWO NEW BRITISH MAMMALS.—At a meeting of the Zoological Society of London on February 7, Mr. G. E. H. Barrett-Hamilton, F.Z.S., read a paper on the Mice of St. Kilda, of which he recognised two species—*Mus hirtensis* sp. nov., a representative of *M. sylvaticus*, and *M. muralis* sp. nov., representing *M. musculus*. Both of these species showed good distinctive characters from their well-known prototypes.

# SOME NEW BOOKS.

## A COMPREHENSIVE WORK ON MOTHS.

Catalogue of the Lepidoptera Phalaenae in the British Museum, Vol. I. Catalogue of the Syntomidae in the Collection of the British Museum. By Sir GEORGE F. HAMPSON, Bart. 8vo, pp. xxi + 559, with 285 illustrations in text, and accompanying volume of 17 plates with coloured figures. London: Printed by order of the Trustees, 1898.

The first volume of this series, the circular relating to which is given in *Natural Science*, vol. xii. 5, is now issued, and will be received with interest by lepidopterists generally. The volume is accompanied by a preface from the Director of the British Museum (Natural History), by a systematic index of the species included, and an introduction of 20 pages from the author. In this introduction the general classification and phylogeny is categorically laid down, and the structure of the Lepidoptera is discussed in a series of brief paragraphs, of which those relating to the ovum, larva, and pupa have been revised by Dr. Chapman and Dr. Dyar. The classification is mainly based upon an interpretation of the neuration. The primary division of Comstock is adopted, and the Micropterygides and Hepialides are opposed to the rest of the moths, because of the 12-veined secondaries, *i.e.* the radius has the same number of branches as on primary wings. More recently *Crinopteryx familiella* has been studied by Spuler and Hofmann. This Tineid represents the final stage in the loss of radial branches, so that a passage is effected between the groups, and the character is vitiated. An ambitious key to the families is given on page 17, in which no use is made of the anal vein of primaries in the Papilionides, while the Satyrids are separated from the Nymphalids by the dilated vein 12. But this vein is dilated in *Potamis iris* (Nymphalidae), and the dilations are reduced in *Agapetes* and *Oeneis* (Satyridae). The more important part of the volume is taken up by the description of the 1184 species of Syntomidae. Purists may write Syntomididae; others, believing the Tentamen edited, and *Glaucopis* being pre-occupied, may prefer Sphecomorphidae, while the Americans use Euchromiadae. The family, from observations on all stages, contains specialised Arctians, the limits of the two groups being obscure. The descriptions, both generic and specific, are concise yet sufficient, but the opportunity of considering the male genitalia has been neglected, which may possibly lead some to regard the work as "superficial." The synonymy often lacks the necessary reference to the author of the combined term adopted, thus breaking the historical sequence (e.g. *Ctenucha virginica*). This fault seems to have been copied, with the citations, from Kirby's catalogue. On page 361 *Burtia* is dated "1867," on the succeeding page "1866," as by Kirby. *Burtia* was published with figure and should have precedence as noted by Moeschler (Lep. Porto Rico, 349), who, however, misplaces the synonym *Gundlachia*. *Horama diffissa*, correctly dated by Kirby, is post-dated, with its synonym, by 20 years. The intention is to include all species agreeing with the family definition. The

figures in the text are recognisable and will be helpful; the coloured figures in the volume of plates merit praise. On Plate X. Fig. 3, not 2, seems to represent *Pseudomya splendens;* such errors are regrettable. The work bears the mark of industry, and can be said to be successfully accomplished, so that the expectation held out in the preface, that it will be of value to the systematic zoologist, may be considered as realised. A. RADCLIFFE GROTE.

## THE PHILIPPINES

The Philippine Islands and their People. By DEAN C. WORCESTER. Roy. 8vo, pp. xx. + 529. London: Macmillan and Company, 1898. Price 15s. net.

Professor Worcester first visited the Philippines in 1887, as a member of Dr. Steere's zoological expedition to that group. He remained upwards of a year, and returned in 1890 for a period of nearly three years. He visited all the important islands, remaining in each long enough to form a fairly representative collection of its birds and mammals. In the present work he does not attempt so much to present an account of his zoological work as to draw a picture of life in the Philippines, and of the condition both of the settled districts and of those occupied by various uncivilised tribes. To the latter, many of whom are practically unknown to ethnologists, Professor Worcester devoted considerable attention, and has made valuable observations on their customs and beliefs. Without setting himself to criticise the Spanish administration, he makes it fairly clear that he did not find it ideally perfect in its relation to individuals, or enlightened in its dealings with the economic development of the islands. At the present moment, when these are on the point of passing into American hands, it is important to note that so good an authority expresses the opinion that it is very doubtful if many successive generations of European or American children could be reared in the Philippines. The climate is exceedingly unfavourable to severe and long-continued physical exertion, such as would be necessary to develop the resources of the islands. An important appendix deals exhaustively with these. The soil is of almost inexhaustible richness, and cacao, coffee, guttapercha, Manila-hemp, bamboo, maize, rice, sugar, tobacco, are among the plants of economic importance. The indigenous mammals are somewhat scanty, but many domesticated ones have been introduced. The large European and Australian horses, however, do not stand the climate. Nearly six hundred species of birds are known, including many rare and beautiful ones. Snakes are numerous, and locusts appear every few years. The most destructive insect pest is a larva which bores the stems of coffee bushes, often destroying whole plantations. Numerous species of fish are found, including a curious fresh-water species which appears annually in the flooded rice fields, vanishing in a mysterious way as the fields dry up. The book is well illustrated, and contains a reproduction of an interesting old map of 1744 and a miserable modern one. A. J. H.

## MR. RUTGERS MARSHALL ON INSTINCT.

Instinct and Reason: An Essay concerning the Relation of Instinct to Reason, with some special Study of the Nature of Religion. By HENRY RUTGERS MARSHALL, M.A. 8vo. New York: The Macmillan Co., 1898. Price 12s. 6d. net.

Mr. Rutgers Marshall is known to psychologists as the author of a work on "Pain, Pleasure, and Aesthetics." He there puts forth certain views on instinct and its relation to impulse; and these are elaborated and extended in the work before us. His main thesis is that the ethical and religious instincts, of profound importance to the human race, have been rendered innate and hereditary

in consequence of their biological value to the members of a social community. There is much that is interesting in his new book; but many of the positions occupied require more weight of fact and more cogency of argument to render them tenable.

Instinct, as defined by Mr. Marshall, includes not only reflex action but, it seems, cellular response to stimulus. For we are told that all instincts, whether simple or complex, fall under one category, and appear as modes of that simplest of all forms of activity, the reaction of a living cell to the stimulus received from the environment. On the other hand, instinct is taken to include all that is the outcome of innate tendencies. Not only are the ethical, artistic, and religious "instincts" comprised within the definition, but reason itself. At any rate, the "important conclusion" is emphasised that "all of reasoned action must be referred back to instinct action." On these principles it is difficult to see what types of organic action, animal or vegetable, must *not* be referred to instinct. It is questionable whether a technical term of such wide meaning has any definite value.

The term "impulse" is applied to "mental phases which, when we take an objective view, we find to be determined by the inhibition of instinct actions." But the word "obstruction" is sometimes used as the equivalent of inhibition. The former word has a much wider meaning than inhibition used as a technical term. It is not clear whether Mr. Marshall's contention is that impulse is always determined by the inhibitory influence of higher on lower nerve centres, or something else.

If we accept the extravagantly broad definition of instinct as comprising rational action, it is clear that the distinction between hereditary and acquired modes of procedure is a superficial one. Mr. Marshall does, however, take it to some extent into consideration. But he seems too readily to accept as hereditary much that may be handed on by tradition.

The last criticism for which we have space is this. It satisfies Mr. Marshall to assume, that if this or that mode of activity is of what he terms biological value to the race, it has been or is being engrained through heredity. Whether he accepts the inheritance of acquired characters is not clear; but he lays some stress on the distinctively Darwinian factor in evolution. He nowhere, however, adequately sets forth the steps of the process by which those who fail to possess, say, the religious "instinct" are eliminated. C. Ll. M.

## BRITISH PARK CATTLE.

White Cattle: an Enquiry into their Origin and History. By R. H. Wallace. Trans. Nat. Hist. Soc., Glasgow, (2) vol. v. pp. 220-273, illustrated.

If the object of this communication were simply to demonstrate that British Park White Cattle are not the direct survivors of the wild aurochs or ox (not the bison), Mr. Wallace might have saved himself the trouble of writing it, since all competent to give an opinion are agreed on this point. But his main contention seems to be to demonstrate their descent from a white breed imported by the Romans. In this view he follows the steps of Professor M'Kenny Hughes; and it is far from our intention to endeavour to demonstrate either that he is right or wrong, for the sufficient reason that, in our opinion, the evidence is inadequate. All that we propose in the way of criticism is to call attention to what appears to us a misapprehension.

In his summary the author states that the cattle common in Britain was the Celtic shorthorn, the so-called *Bos longifrons*. This animal is stated to have been small and dark coloured. At the time in question the aurochs (*Bos primigenius*) had become exterminated in Britain; while the Celtic shorthorn was domesticated. The Romans had a special breed of white cattle for sacrificial purposes; and such cattle were brought into Britain. It is from these cattle

that the British Park breeds are derived—perhaps with some amount of crossing.

Such appears to be, very briefly, the author's argument. Putting entirely on one side the question of the particular domestic breed from which the park cattle are descended, we at once proceed to our criticism. This is based on our belief that the author does not know what he means by a species. He says[1] that "our common cattle, *Bos taurus*, is *no doubt* a mixed product of extremely numerous and very diverse factors, developed in widely-separated regions. This animal *when wild* was probably hunted by man, but, tamed, it has accompanied him in all his wanderings. Its geological history in Britain, according to Owen, is first a large species of ox, *Bos antiquus*, followed by a somewhat smaller but still stupendous wild ox, *Bos primigenius*, succeeded in turn by an aboriginal British ox of much smaller stature with short horns, *Bos longifrons*." Later on, p. 245, he observes: "Practically we can entirely ignore *Bos primigenius* as a factor in the history of early British cattle, especially of white breeds."

Now the greater part of the above appears to us pure nonsense. What, for instance, does the author mean by saying that our domestic cattle had a very complex origin, and then that it once occurred *wild?* Are different species of wild animals in the habit of producing a mixed breed for the special benefit of man, that he may capture and tame it?

As a matter of fact, there are only two possible origins for European domestic cattle (as the bison and buffalo may be put aside), namely, the aurochs and the extinct Narbada ox of India. And there is little doubt that the honour of parentage belongs to the former. As to the Celtic shorthorn there is no evidence that it was ever a wild animal. Moreover, as it is not specifically separable from *Bos taurus* (typified by the domestic ox of Europe, which is also the type of its genus), and never having been wild, it cannot claim to be termed a sub-species or race. Neither is there any possibility of specifically distinguishing the Plistocene aurochs from the aurochs of the Middle Ages; although as this was a true wild animal it is entitled to rank as a sub-species, *Bos taurus primigenius*. That this *Bos taurus primigenius* was the proximate ancestor of the Celtic shorthorn, and hence the ultimate ancestor of all European breeds, is, we submit, beyond doubt, unless indeed the aforesaid shorthorn fell from the stars or was separately created! In place, therefore, of the so-called *Bos primigenius* having nothing to do with the origin of domestic cattle, we believe it has everything to do therewith. To look to the Indian humped ox as the ancestor of some at least of our domestic breeds does not help matters. Ordinary and humped cattle (although they will readily cross), differ by their form, voice, and habits; and, although some crossing may possibly have occurred in certain districts, there is not a tittle of evidence in favour of this origin, while there is everything against it. If such were the origin, *Bos indicus* would, of course, have to be sunk in the earlier title *Bos taurus*.

One word more and we have done. On page 222 it is stated that Mr. Lydekker favours the view of the origin of British park cattle from white Roman cattle. Now we have a fairly good acquaintance with the somewhat discursive writings of that gentleman, but have failed to find in any of them *bearing his name* authority for such a statement.

## DESCENSUS TESTICULORUM.

Studien über Säugethiere. By Dr. Max Weber, Professor of Zoology in the University of Amsterdam. Part II. 8vo, pp. 132, with 4 plates and 58 text figures. Jena: Gustav Fischer, 1898. Price 12 marks.

This book follows its first part after an interval of ten years, but even that length of time does not seem too great for the preparation of the mass of infor-

[1] The italics are ours.

mation which it contains, although the author explains that he issued the book on the eve of assuming the leadership of a Netherlands Deep Sea Expedition to the Indian Archipelago.

With the exception of a few pages the work is devoted to a study of the descent of the testicles of mammals, for which the mere collection of the necessary material could not have been accomplished without the exercise of much patience.

At the outset, Dr. Weber gives an admirable summary and criticism of the diverse structures which different writers have included under the term "gubernaculum," and suggests that this word should be allowed to fall into disuse, since "es hoffnungslos ist, das Wort Gubernaculum zu gebrauchen, ohne dass es Anlass zu Missverständnissen giebt." He prefers to use terms which have a precise significance, such as ligamentum testis, ligamentum inguinale (lig. rotundum), conus inguinalis, cremaster sac, etc.

Details are given of dissections of animals representative of all the mammalian orders, followed by chapters, on the position of the testis, the position of the vasa deferentia, the ligamentum inguinale, and the chorda gubernaculi.

In a very interesting table the author distinguishes between (1) mammals whose testes lie at least temporarily external to the true abdominal cavity, and (2) those whose testes remain permanently within the abdominal cavity. To this latter group he applies the name "Testiconda," and again subdivides it into (*a*) those in which the inguinal canal and the ligamentum inguinale are wanting—True Testiconda—and (*b*) those in which the inguinal canal is obliterated to different degrees, and the ligamentum inguinale at the most is still present in rudiment—False Testiconda.

In discussing these conditions Dr. Weber considers that the true testicond mammals acquired the characteristic at a time when the descensus was still in process of evolution, and as yet only a slightly stereotyped arrangement. Among the Marsupials and the majority of Monodelphia this new arrangement improved more and more, the different stages being still existent among recent mammals; while Testiconda became the stereotyped arrangement with isolated ones, others (Cetacea, Dasypodidae) lost the descensus which had already reached completion, and thereby became Testiconda secondarily. From this point of view true Testiconda ought to be regarded as a return to the primitive condition, not as a return to embryonic conditions, although it must be granted that the embryonic condition is a reiteration of the primitive.

The remainder of the book is taken up with three interesting papers on the elephant, in which the construction of its feet, the peripheral organ of smell, and its brain are respectively dealt with. D. H.

## A WIDE RANGE.

Biological Lectures, delivered at the Marine Biological Laboratory of Wood's Holl, 1896-97. 8vo, pp. 242, with figures. Boston: Ginn and Co., 1898. Price 8s. 6d.

A Course of Lectures whose aim is merely defined as being the free discussion of "unsettled questions" in Biology, must necessarily, we suppose, allow to individual lecturers considerable freedom of choice. Certainly the eleven lectures contained in the present volume range over a large number of subjects, and treat these from very different standpoints. From the literary point of view two papers are especially noticeable—one by Prof. C. O. Whitman on "Some of the Functions and Features of a Biological Station," and the other on "The Methods of Palaeontological Inquiry," by Prof. W. B. Scott. Prof. Whitman's paper is a charming discussion of the ideal biological station, which incidentally involves scathing criticism of modern methods, and ends in the

building of a fair palace, with lakes and aquaria, experimental gardens and well-equipped laboratories, where the over-specialised worker may once more get into touch with nature. Prof. Scott devotes himself to a description of the methods employed in modern palaeontological research, taking as an example the fossils of the White River beds. These are traced from the moment of their exhumation to the final setting up of the restored skeleton. The paper is illustrated by photographs, some of which are, unfortunately, somewhat spoiled in reproduction, and is an admirable example of what a popular lecture should be—and so rarely is.

The other papers in the volume are of a more technical nature, and in most cases display little attempt at literary effect. Mr. Hermon C. Bumpus has an interesting paper on the variations of *Passer domesticus*, the introduced sparrow. A comparison of 868 American, and the same number of English eggs, showed that the American eggs were much more variable both in colour and in shape than the English, and further, that the mean shape of the American eggs is different from that of the English eggs. The result is interesting, but we wish that Mr. Bumpus had stated the localities from which the eggs were obtained; the American area is obviously somewhat larger than the British area, and this introduces a possible source of error. Another interesting, though speculative, paper by Dr. Arnold Graf on the physiology of excretion is an attempt to explain the phenomena of excretion in leeches on "mechanical" grounds. Two papers on the centrosome, by Miss Foot and Mr. A. D. Mead respectively, are of interest because their authors both reject Boveri's hypothesis of the great importance of the centrosome in cell-division. An elaborate paper by Dr. Conklin on "Cleavage and Differentiation" is marred by the obscurity of the style and want of care in arrangement. The remaining four papers are devoted to a variety of subjects.

As a whole, in spite of much that is good, we confess to finding the volume a disappointment. There is no index, in most cases not even connected lists of references, and the absence of an editor is often painfully apparent. Many of the lecturers seem to us to show lack of discretion both in their choice of subjects and in their treatment of them under the given circumstances. Finally, the very miscellaneous nature of the contents produces a somewhat painful effect, slightly suggestive of Dr. Blimber's establishment. N.

## THE COLOURS OF ANIMALS AND PLANTS.

Colour in Nature. A Study in Biology. By Miss MARION I. NEWBIGIN, D.Sc. 8vo, pp. xii. + 344. London: John Murray, 1898. Price 6s.

Miss Newbigin has performed a useful piece of work in placing before us a book upon colour in nature which abounds in fact and is not dominated and rendered ineffectual by theory. Out of fifteen chapters only one, the last, is devoted to theory; and that seems to us to be an exceedingly reasonable proportion. There is perhaps no department of biology in which so extraordinary a disproportion between fact and theory obtains as in that relating to the colours of animals. To give an example, even the first year's student at a medical school has some notion of the complex structure of the visual organs of the insect and crustacean tribe, and the difference from the analogous organs of the vertebrates. His teacher, if he be a wise person, declines to call the former "eyes": he prefers to term them "visual organs," not in the least on the principle of the journalist who writes of "vehicular traffic," and "the devouring element," but simply because he will not prejudge the question of their correspondence with what have been always called eyes, that is, the eyes of vertebrated animals. Yet the reader of any book or article upon colours in animals and plants is requested to believe—or rather it is assumed without any request at all that he *does* believe—that the physiology of the insect eye

and the visual judgments in the "brain" of an insect are precisely similar to those, not merely of a man, but of an educated man accustomed to observe accurately and reason. A person really competent to theorise upon such matters would have to be—as John Evelyn described Edward the Sixth—"stupendiously knowing." The real fact of the matter is, that Miss Newbigin is perfectly right in pointing out that we do not yet know enough of the plainest phenomena of animal coloration. "We cannot," she justly observes, "end a book on colour more fitly than by an appeal for more facts." The authoress herself deals with the main pigments and the physical causes of colour in the animal world. The book is a valuable epitome of such facts, and will doubtless be read by both students and the general public. F. E. B.

## A GEOLOGIST'S ALMANACK.

Kalender für Geologen, Paläontologen und Mineralogen. Herausgegeben von Dr. K. Keilhack. Leipzig: Max Weg, 1899. Price 3 Marks.

The second edition of this compact and useful pocket-book is an improvement on its predecessor, and should prove a valuable companion to every active or professional geologist. It contains the following sections: I. The Government Geological Surveys of the world. Every country in Europe has a survey except Greece, Turkey, Servia, Bulgaria, Montenegro, and Holland; in Asia, only India and Japan have one; in Africa, only Egypt, Cape of Good Hope, and the Transvaal; in South America, only Brazil; in North America, Mexico should have been mentioned, but is not; in Australia, New Zealand is included, but the active survey of Queensland is omitted. II. List of teachers of the geological sciences in the colleges of the world. III. Geological, mineralogical, and palaeontological societies. IV. Addresses of the geologists, mineralogists, and palaeontologists of Germany, the Netherlands, Austria, Switzerland, and Hungary. There have also crept into the list a few people in other places, such as London, La Plata, St. Petersburg, and Guatemala. V. The public and private geological, mineralogical, and palaeontological collections of the same countries. VI. Comparative lists of geological formations. This is very incomplete, being confined to Europe, and dealing with that in a partial manner. At least one would expect to find the classical divisions of the British Silurian. VII. Synoptic table of igneous rocks. VIII. The chief characters of the more common minerals. IX. Symbols for crystal faces, according to Naumann, Weiss, and Miller. X. Atomic weights of the elements. XI. History of the names of the chief formations, by J. Walther. XII. Rules for the application of proper names in systematic nomenclature, by H. Potonié. We have no patience with a person who says that the genitive of "Martius" should be "Martiusii." But in this branch of learning the best way of avoiding puerilities is never to use personal names at all. XIII. Short account of the annual gatherings of the German Geological Society, the Oberrhein Geological Society, and the International Congress. XIV. A list, very incomplete, of geologists, mineralogists, and palaeontologists dead since October 1, 1897. XV. Chief measures of length reduced to the metric scale. XVI. Chart of magnetic declination in Europe during 1899. XVII. List of periodicals containing geological papers. XVIII. The geological, palaeontological, and mineralogical literature of 1898 down to the end of November. This, though it contains over 900 titles, and is much to be grateful for, is by no means complete.

There are the usual diary, blank pages, and section paper, as well as a portrait of the late C. W. von Guembel. Considering the great difficulty of attaining accuracy in a work of this kind, we may well compliment the editor and publisher on what they have done for us. B.

## PETIT CULTURE.

Bush Fruits. A Horticultural Monograph of Raspberries, Blackberries, Dewberries, Currants, Gooseberries, and other Shrub-like Fruits. By FREDERICK W. CARD, Professor of Horticulture, Rhode Island College of Agriculture, etc. 8vo, pp. xii. + 537, with 113 figures. London and New York: Macmillan and Co., 1898. Price 5s.

The scientific cultivation of fruit, especially of the smaller kinds, has at least in this country not received the attention which it deserves. It may indeed be doubted whether many of those who delight in the autumn wealth of our English hedgerows think seriously of growing the wild species for profit, much less of attempting to improve them. Our American cousins have, however, devoted a considerable amount of characteristic energy to the cultivation of "berries," whether native or foreign, and the present volume is the first of a proposed series of monographs on American fruits.

Professor Card divides his work into four parts, the first of which is devoted to the discussion of the cultural methods best adapted to berries in general, with remarks on marketing and evaporation. The second and third parts are concerned with "brambles" (including raspberries) and "groselles" (currants and gooseberries) respectively, and contain special directions for the cultivation of the various species and varieties. Careful descriptions and figures are provided of the more important pests and diseases, while references to standard works are given for all the species of fungi and insects known to attack the plants concerned.

The book should prove of service to all who are interested in petit culture, whether in America or at home, not only because certain of the species described are native to those islands, but also because similar treatment might with advantage be applied to some of our own wild fruits. J. A. TERRAS.

## INTRODUCTION TO PHOTO-MICROGRAPHY.

Photo-Micrography. By EDMUND J. SPITTA, L.R.C.P., M.R.C.S., F.R.A.S. 4to, pp. 163, with 41 half-tone plates and 63 text illustrations. London: The Scientific Press, 1899. Price 12s.

This work will be useful not only to beginners, but also to those accustomed to photo-micrographic work. It discusses the methods employed, the difficulties encountered, and the means adopted to overcome them. Of especial interest are the parts dealing with the covering power of lenses, the best combinations to use under special circumstances, the employment of condensers, the rough estimation of the N.A. of low power lenses, bacterial cultures and slides, and Appendix IX. It is to be regretted that the author makes no mention either of magnesium as an illuminant nor of the methods available in the case of living specimens. A more detailed account of the best methods of producing fine negatives of histological specimens with the highest powers would have added much to the practical value of the work. The figures, with the exception of the histological ones, are for the most part excellent. E. W. C.

## THE ANTLERS OF THE CERVIDAE.

Studien über Hirsche (Gattung Cervus im weitesten Sinne). By Dr. H. NITSCHE. Heft 1. 4to, pp. xii. + 102, with 12 pls. and 12 figs. Leipsic: Engelmann, 1898. Price 20 Marks.

The importance of antlers in the classification of the Deer, although somewhat underrated a few years ago, is now fully recognised by naturalists; and it is therefore satisfactory to find a morphologist of Dr. Nitsche's standing taking up the subject in earnest. The range of antler variation has indeed been fully

mastered from a classificatory point of view; and much has been done in homologising the various constituents of these appendages in the different genera and sub-genera. Much, however, still remains to be accomplished in this aspect of the subject, especially as to what extent the antlers of the Old World and New World deer (*Mazama*) are really homologous, and the degree of evolution they had attained when the two groups diverged.

In the present part, which treats solely of antlers, and their relationship to horns, the author takes into consideration abnormal developments of the former appendages, which are discussed with great elaboration and wealth of illustration. But the conclusions to be drawn from these abnormalities are reserved for future consideration, so that it is impossible to formulate, let alone criticise, the author's views until the work is further advanced. As Dr. Nitsche attended the Cambridge Congress last August, he had an opportunity of seeing the collections on which recent English work has been based, and was much impressed by their extent and completeness.

## A FIFTEENTH EDITION.

Determinative Mineralogy and Blowpipe Analysis. By GEORGE J. BRUSH. Revised and Enlarged by SAMUEL L. PENFIELD. Fifteenth edition. 8vo, pp. 312, with 375 figures. New York: John Wiley and Sons. London: Chapman and Hall, Ltd., 1898. Price 15s.

Like other branches of science, mineralogy has for many years past been making rapid advances, and the work of acquiring the most recent information often entails the expenditure of much labour and time. On this account students of mineralogy will gladly welcome the appearance of this new edition of an already well-known book which it is unnecessary to praise. Suffice it to say that its past reputation is amply sustained in the present work. In it the most recent methods of blowpipe analysis are fully and clearly described. In chapter iii. the reactions of the elements are given in alphabetical order, and in the following chapter the various modes of procedure in blowpipe analysis are systematically set forth, the descriptions being accompanied by well-printed tables free from abbreviations.

Chapter v. is devoted to the physical properties of minerals, beginning with crystallography, which is dealt with in sixty-five pages. In this the now generally accepted treatment of symmetry is adopted, and the employment of crystal forms instead of spherical projections will probably enable the beginner to grasp this part of the subject more readily than he would do from the illustrations which have hitherto been used. The figures in this chapter are well executed. It might, however, have been indicated on p. 222 that a radiating fibrous or divergent crystalline structure is common to all the four minerals selected for illustration. The chapter concludes with a few pages on the different methods of determining specific gravity.

Chapter vi. consists of a most valuable series of tables for the determination of mineral species, thoroughly well brought up to date. These cannot fail to be of infinite use both to teachers and students. For the benefit of the latter the names of the most important species are printed in heavy type. A good index to general matter, followed by an index to mineral species, concludes this admirable and most useful book. F. R.

## FERTILITY OF FARM AND GARDEN.

Fertilizers. By EDWARD B. VOORHEES, A.M. 8vo, pp. 335. London: Macmillan and Co., 1898. Price 4s. 6d.

The soils of America, like those of older countries, become exhausted in time by crop cultivation. We have increasing evidence of this in the multiplication of American works on fertilizers and the like. Of these the present

treatise is a good sample. While details applicable to the cultivation of sweet potatoes, millet, and cotton may be of little service to the British farmer or market-gardener, he will here find fresh light thrown on many matters of direct interest to him. The nature, uses, and value of staple artificial manures, as well as less familiar ones, such as "ground king crab" and "garbage tankage," are lucidly and popularly dealt with. We fully acknowledge that we have much to learn, and to unlearn also, in regard to the action of fertilizers. Experiments such as are being carried out at the American Experiment Stations are the surest means of affording the desired knowledge. We are again reminded of our need of similar institutions here.

JOHN H. WILSON.

Recent Advances in Astronomy. By ALFRED H. FISON, D.Sc. 237 pp. The Victorian Era Series. London: Blackie and Son, Limited, 1898. Price 2s. 6d.

To give in the scope of so small a book anything approaching an exhaustive account of the great achievements in astronomical science during the last sixty years would be far from possible. The author therefore confines himself to describing what are in his opinion some of the more important discoveries of recent times. It is at once evident that the greater part of the contents had thus to be devoted to a description of the splendid advances in spectroscopy, and in those fields of astronomical research which have thereby been opened to human thought. Here we find an instructive and interesting historical description of what has been done since the all-important invention of the spectroscope. One of the best chapters deals with the development of our knowledge of Mars, which, chiefly since Schiaparelli's discoveries, has become an object of earnest inquiry as well as of the keenest controversy. There is also an account of the investigations made with regard to stellar parallax, the distribution of stars in the heavens, their life in the past, and their probable future. Although allowance must be made for a difference of opinion as to whether the author has, on the whole, really selected what astronomers may think the most important advances made in astronomy during the Victorian Era, we cannot but admire the lucidity and grasp which he shows in his exposition of the subject. J. HALM.

*The Educational Review* is further entitled "A Magazine of the Science and Art of Education, and Review of current educational Literature and Events." It is henceforward to be published on the 8th of each month at 203 Strand, London, W.C., price 4d. A new series began with the January number. The object is to discuss the principles rather than the politics of pedagogy. The number sent us contains valuable and suggestive articles by Miss Beale, Canon Lyttelton, Dr. Sophie Bryant, and others, but nothing of special interest to scientific readers.

The Geological Survey of Queensland has issued as *Bulletin* No. 10, "Six Reports on the Geological Features of part of the District to be traversed by the proposed Transcontinental Railway," by R. L. Jack, Government Geologist. These were originally printed as a Parliamentary Paper in 1885, and are now reprinted with corrections and footnotes. *Bulletin* No. 9, also by Mr. Jack, is a "Report . . . on the Chillagoe Mining District and projected Railway." *Bulletin* No. 8 is a "Report on the Gold Mines at the Fanning and Mount Success (with map)," by W. H. Rands.

Dr. Gadow has been kind enough to point out an unfortunate mistake in our last number. "The Last Link" was said to cost 3s. 6d., whereas the price is 2s. 6d.

# OBITUARIES.

## HENRY ALLEYNE NICHOLSON, M.D., D.Sc., F.R.S., ETC.

Born 1844; Died January 19, 1899.

The name of Henry Alleyne Nicholson has been so long familiar to successive generations of students of zoology and palaeontology, that it must have been a surprise to many to learn that his life had reached its close in only its fifty-fifth year.

His father, Dr. John Nicholson, was distinguished as a Biblical scholar, and was the son of a former President of Codrington College, Barbados. H. Alleyne Nicholson was born at Penrith on September 11, 1844, and received his school-training at Appleby Grammar School. He was also for a time in the hands of Francis Newman. He commenced the systematic study of natural science in the University of Göttingen, and from 1862 till 1867 he was a student of science and of medicine in the University of Edinburgh, graduating B.Sc. in 1866, and M.B. and C.M. in 1867. The study of medicine at that period offered the best access to the higher educational positions in natural science, and it was for this reason that he appears to have entered upon the study. In both science and medicine he was a most successful student, gaining high honours in the several classes. In 1866 he received the Baxter Scholarship as the most distinguished graduate in science for the year, and in 1869, on taking the degree of M.D., he was awarded the Ettles Medical Scholarship as entitled to the highest place for the year among the medical graduates of Edinburgh.

While yet a student he had turned his attention to the geology of his native district, and had studied it with such care that he received the gold medal for his doctorial thesis "On the Geology of Cumberland and Westmoreland." Devoting himself to natural science, Nicholson became, in 1869, Lecturer on Natural History in the Extra-mural Medical School in Edinburgh, and delivered courses of lectures in zoology and in geology during about two years. In 1871 he was offered, and accepted, the Professorship of Natural History in Toronto, in Canada. In 1874 he was appointed, almost at the same time, to the Chair of Comparative Anatomy and Zoology in the Royal College of Science, Dublin, and to that of Biology in the College of Science, Durham. He chose the latter, and performed its duties during two sessions. In 1875 he was offered the Chair of Natural History in the University of St. Andrews, and he held it until 1882, when he succeeded Professor Cossar Ewart in the Chair of Natural History in the University of Aberdeen. At that time the winter course in Aberdeen, attendance on which was compulsory for graduation in Arts, consisted of a hundred lectures, chiefly on zoology, the last two months or so being devoted to geology. The summer course, on zoology alone, was suited more especially for medical students.

In consequence of the changes rendered necessary in the curricula by the Ordinances of the Scottish Universities Commission, geology acquired a more

adequate status in the University of Aberdeen, being placed on an equality with the other natural sciences.

The teaching of geology now required provision for elementary and advanced work, both theoretical and practical, and Professor Nicholson applied himself with much zeal and success to securing the necessary equipment. The classes were greatly appreciated by the students, a fact that gave him much pleasure. He was relieved of the work of teaching zoology, except for the lectures in summer, his assistant, Dr. Alexander Brown, being appointed by the University Court lecturer in zoology, with the charge of the rest of the instruction in that department.

As a teacher Professor Nicholson was singularly successful. An adept in the sciences which he taught, he was not content to rest upon his stores of knowledge, but made it his practice to revise immediately beforehand the subject on which he was to speak. Possessed of natural fluency, and knowing clearly what he wished to communicate to his hearers, he secured their interest and admiration. He excelled as a draughtsman with chalks, and his lectures gained much by his free use of the blackboard. His success as a teacher was largely aided also by personal qualities, a keen but always kindly sense of humour and great geniality of temper, that won for him the love of his students and colleagues, and has made his death to be felt as that of a friend.

Dr. Nicholson was by preference a palaeontologist, and he did much original research in this field, the results of which have chiefly appeared in numerous lengthy and valuable contributions to the publications of scientific societies. Among these, which commenced with papers on Graptolites in 1866 and 1867, may be named particularly his "Monograph on the British Graptolitidae" (1872); "Reports on the Palaeontology of the Province of Ontario" (1874-75), published by the government of Ontario; "Monograph of the Silurian Fossils of Girvan, Ayrshire" (1878); "The Structure and Affinities of the Tabulate Corals of the Palaeozoic Period" (1879); and "Structure and Affinities of Monticulipora" (1881). As indicating the extent of his labours, we find in the Royal Society's "Catalogue of Scientific Papers" that up to 1883 he is named as sole author of seventy-five papers and as joint author of nineteen others. These numbers have since been considerably increased.

He was perhaps better known to students as the author of highly valued manuals and text-books on zoology and on palaeontology. The "Manual of Palaeontology," by Professor Nicholson and Dr. Lyddeker, reached its third edition in two large volumes in 1890, and has received very wide recognition. He also contributed zoological articles to the "Encyclopaedia Britannica," and was the author of a more popular work entitled "Natural History, its Rise and Progress in Britain."

In 1877, and again in 1890, he was appointed Swiney Lecturer in the British Museum, and in that capacity he delivered lectures on geology and palaeontology to large audiences with great acceptance.

During the past six years his health had not been good. Though not unfitted for the duties of his office he suffered from frequent attacks of influenza, with complications, which at the beginning of the present winter took the form of gastralgia. After a short period of severer illness he died on the 19th January.

He has left a widow, two daughters, and three sons. His sons have already gained distinction in their several paths. J. W. H. TRAIL.

## WILLIAM COLENSO.

BORN, 1811; DIED, FEBRUARY 10, 1899.

THE Rev. William Colenso was born at Penzance. He started in life as a printer and bookbinder in the office of Watts and Son, London, where he was

engaged on work for the British and Foreign Bible Society. This early training stood him in good stead, for in 1833 he was sent out by the Church Missionary Society to establish a press in New Zealand. Here he spent his long life, devoted to missionary work mainly, and to botany, zoology, and ethnology secondarily. His first paper, on botany, was published in 1842 in the *Tasmanian Journal of Science*, and since that date he had contributed 32 papers on the above subjects to various periodicals. Colenso was an enthusiastic collector, and supplied Richard Owen with much information concerning the Moa and other extinct vertebrates. His knowledge of the Maori, his antiquities, and myths, was second to none.

## THOMAS HINCKS, B.A., F.R.S.

Born at Exeter, July 15, 1818; Died at Clifton, January 25, 1899.

The well-known author of "A History of the British Hydroid Zoophytes" (1868), "A History of the British Marine Polyzoa" (1880), and a long series of papers on marine zoology, most of which appeared in the *Annals and Magazine of Natural History*, was the son of the Rev. William Hincks, formerly professor of natural history at Toronto. From 1855-1869 he was minister of the Mill Hill (Unitarian) Chapel at Leeds, where he took an active part in public and philanthropic affairs, turning his leisure meanwhile to marvellously good account in zoological work, which is a model of painstaking accuracy and sound judgment. Failure of the voice compelled him to abandon his ministerial work, but he continued his scientific researches with zest almost to the end. At Taunton, and afterwards at Clifton, he lived his quiet life, gardening and observing and helping other workers. He was the last survivor of that illustrious company to which Professor M'Intosh recently referred (*Nat. Sci.* vol. xiv. p. 76) in his obituary notice of Allman.

The deaths are also announced of Professor Dareste de la Chavanne, of Paris, the well-known teratologist; F. Gay, of Montpelier University, a student of the green algae, aged 40; Gilbert H. Hicks, First Assistant Botanist and Seed Expert of the United States Department of Agriculture since 1894, for many years an editor of the *Asa Gray Bulletin*—he left a work on seeds shortly to be published by the Macmillan Co.; Major J. Hotchkiss, author of a number of papers on economic geology; Dr. Franz Lang, teacher of natural history at, and rector of, the Cantonal School at Soleure, Switzerland, aged 78; Professor Gianpaolo Vlacovich, anatomist, at Padua. The Aberdeen students' Magazine *Alma Mater*, for January 25, contains affectionate appreciations and a good portrait of the late Professor Alleyne Nicholson

# CORRESPONDENCE.

Sir—Mr. Bulman's paper in your February number reopens a very interesting question. In the paper which he criticises, I contended (1) that nearly allied species are intersterile, and (2) that this being granted, we may put down to the credit of insects the development of flowers. To make good my first contention I depended mainly on the bees themselves; they not infrequently wander from species to species, and yet no intercrossing takes place. Thus I am able to dispense with the evidence supplied by the experiments of Alexis Jordan. If, however, his conclusions are accepted, my case becomes still stronger, since intersterility is extended even to sub-varieties. If it be conceded that species are sterile *inter se*, it is not difficult to show the reasonableness of my second contention. The plants compete for the visits of bees, and the bees are in the position of a gardener who isolates a particular species, selects his plants of brightest bloom, and sows seed only from them. This assumes that bees have a colour-sense. The experiments by which Prof. Plateau attempted to prove that they had none really proves nothing of the kind, as Sir John Lubbock made clear in the *Journal of the Linnaean Society* (April 1, 1898). It is true that scent may answer the same purpose as colour, but colour or fragrance a plant must have or else bees will leave it unvisited. As to the term by-product, I meant that the plants from which our phanerogams are descended produced colour, but turned it to no useful purpose. It has been fostered and developed through natural selection.

There remains a point which Mr. Bulman has raised and which I certainly dealt with inadequately. "Admitting," he says, "that all species, sub-species, and varieties, as they exist to-day, are sterile *inter se*, we cannot suppose that the *varying individuals* in a species—which must form the beginning of a new species—are so." In this almost all biologists will agree with him. It is impossible to accept the theory of physiological selection in the form in which Romanes propounded it. If a few individuals in a species, having no superiority to their fellows and no distinguishing mark, are fertile only *inter se*, they are not likely to leave descendants: with flowering plants, since in many species cross-fertilisation is required only occasionally, the chances are much better. The few "physiologically separate" individuals might, by means of self-fertilised ovules, so increase their numbers as no longer to be scattered units among the herd from which they have cut themselves off. Setting aside this possibility, we can appeal to geographical isolation to help us. Since individual plants are fixed in one spot, such isolation may easily arise and continue long enough for a variety, originated locally, to become sterile with other varieties or with the parent species. Such local forms might arise in neighbouring valleys or on the banks of two streams that flowed not far apart. Armed with the intersterility thus obtained our young species will extend their range and settle among allied species and varieties without danger of intercrossing, and the bees will be able to pursue the work of developing their flowers.

F. W. Headley.

Haileybury College, Hertford.

# NEWS.

The following appointments have recently been made:—Dr. Angelo Andres, well known for his work on sea-anemones, to be professor of zoology and comparative anatomy in the University of Parma; Dr. H. E. Annett, as demonstrator of tropical pathology in Liverpool; C. Gilbert Cullis, to be assistant professor in the Geological Department of the Royal College of Science; Dr. D. T. MacDougal, of the University of Minnesota, to be director of the laboratories of the New York Botanical Gardens; G. F. Stout, lecturer on comparative psychology at Aberdeen, to the recently founded Wilde lectureship in mental philosophy at Oxford; Vidal de la Blache to be professor of geography in the University of Paris; German Sims Woodhead, M.D. (Edin.), to be professor of pathology at Cambridge, in place of the late A. A. Kanthack.

Walter Myers, M.A., and E. S. St. B. Sladen, M.A., both of Gonville and Caius College, have been elected to John Lucas Walker studentships of the University of Cambridge; J. Stanley Gardiner, M.A., Fellow of the same college, has been elected Balfour student for three years from March 25, 1899.

Dr. Roux has been elected a member of the Paris Academy of Sciences in the section of rural economy.

Dr. Richard Garnett, C.B., keeper of the printed books in the British Museum, has resigned his position, after a connection with the institution of forty-eight years. His name will always be associated with the monumental catalogue which has placed the literary treasures of the Museum within the reach of all, and his personality will ever be remembered for unfailing courtesy and readiness to aid with his own almost unrivalled knowledge the humblest student who asked his assistance. There are many students of natural science who have had the advantage of his marvellous bibliographical erudition.

On February 2 a new bacteriological institute on a large scale was opened at Louvain.

The annual meeting of the Millport Marine Biological Association was held in Glasgow on February 9. The honorary treasurer, Mr. Alexander Somerville, submitted the annual report by the Committee of Management, which gave an account of the first year of the actual working of the marine biological station at Millport. There were over 8000 visitors to the museum during the past year, and tables in the laboratory were utilised for terms varying from a week to a month on thirty-eight different occasions. The Committee took this opportunity of tendering very hearty thanks to all who had contributed in any way to the welfare of the station. During the past year many additions have been made to the station, especially in the laboratory department, but much is still required. The station cost £1800, but the Association has paid its way, and the balance against it is at present only £153, which says much for the good management.

An experiment will shortly be made by a few interested in botany, towards establishing a collection of living British plants for purposes of study and

observation. The garden will be a small strip of land, 200 ft. × 80 ft., at Castlands Road, Perry Hill, London, S.E., and will be under the charge of Mr. P. Cochrane. According to the plan, which we have seen, the land will be laid out with soils suitable to special classes of plants, as clay, sand, chalk, etc., and there will be marshy tracts to suit on the one hand fresh-water plants, and on the other those that live near the sea. There will also be fresh and salt water pools. Small zoological and geological collections are proposed later. The idea is being pushed forward by Mr. W. H. Griffin, the Hon. Sec. of the Catford Natural History Society, Mr. A. A. Abbott of Perry Hill, and others. The main object of the promoters is to secure for their district an educational exhibit, which will be, should it prove successful, of considerable value, and in any case cannot fail to promote an interest in botany. Further particulars can be obtained of Mr. Cochrane, at 47 Perry Hill, S.E.

Dr. Melchior Treub, director of the botanical garden at Buitenzorg, recently celebrated the twenty-fifth anniversary of his doctorate. This has formed the excuse for the issue of a supplement to the *Annales* of that garden, which, in 167 pages with 9 plates, contains 23 papers written by some of the botanists and zoologists who have worked at the garden.

Cambridge University has bought the Carne collection of Cornish minerals for £475.

There has recently been erected at Great Crosby, near Liverpool, an erratic of gypsum, which was found in the boulder clay of that place. The erratic weighs about fourteen tons, and measures 9 ft. 6 in. × 7 ft. 4 in. × 5. ft. 7 in. The occurrence of gypsum is rare in these deposits, and Mr. Mellard Reade, who describes the specimen in a separately printed tract, is of the opinion that it probably came from Whitehaven, in Cumberland. The erratic was presented to the town by Mr. Edward Peters, and the District Council of Great Crosby have shown considerable public spirit in permanently preserving this interesting geological object lesson. The boulder has been erected in the precise position in which it was found.

Dr. E. J. Nolan has, says *Science*, presented the Philadelphia Academy of Natural Science with five volumes in memorial of the late Dr. Joseph Leidy. The first contains biographical notices and similar material; the second contains botanical drawings and notes by Dr. Leidy, and the remaining three his zoological drawings and notes. All are carefully indexed.

The latest of the Museum Handbooks (Publication 24) of the Manchester Museum, Owens College, is somewhat of an "omnibus" nature. It is devoted to "The Marine Mollusca of Madras and the immediate neighbourhood. Notes on a collection of marine shells from Lively Island, Falklands; and other papers. By J. C. Melville, M.A., F.L.S., and R. Standen." The "other papers" are really three short notes on individual species, and the whole, illustrating specimens in the museum, is reprinted, with the two plates, from the *Journal of Conchology*, vol. ix., "in the hope," which we cordially echo, "that they may prove useful to those who study the collections of Mollusca in the Manchester Museum."

This is not the first time that the museum authorities have drawn on the same Society for materials to form one of their excellent series of guides, and the principle is one which other kindred institutions would do well to copy, since publication by reprint must be an economical mode of publication. The original pagination has been scrupulously adhered to, as should always be the case in a reprint, but it spoils the look for a Handbook, and we would suggest to the museum authorities that appearances would be greatly improved if in future productions of this nature the original pagination were transferred to the inner end of each headline and an independent pagination inserted in its place. The cost would not be more than the finances of the College could well bear.

We have before now alluded to the educational collection of natural history specimens and literature relating thereto, belonging to Mr. S. Prout Newcombe, and at present displayed in the Free Library of St. George's, Hanover Square, London. The space is now required by the library commissioners for the regular purposes of the library. Mr. Newcombe now offers the collection to the London County Council under the following conditions :—(1) The collection is to be kept and exhibited always in a room to be named "The Natural History Reading Room," which room is to be maintained in good order at the expense of the Council, and the collection is to be kept separate and distinct from other than natural history objects and literature; (2) the collection is to be transferred to the Council for educational purposes, and not as a public exhibition; (3) before the end of the present year the collection is to be removed temporarily to the Shoreditch Technical Institute, and within a certain period thereafter to be transferred to a suitable place in the county of London. The "suitable place" may be the Chelsea Physic Garden, which, as we have already recorded, is to be made available for science students. The Technical Education Board has suggested the great hall of Aske's Schools, which would gradually be equipped as a museum in connection with the cabinetmaking classes. Whatever plan be finally decided on, we beg to urge that the exhibit should be made readily accessible to the wider circles of the public, for whom it is intended, and that no attempt should be made to give it a severely scientific or technical character.

Mr. Edward Austin of Boston, an East India merchant, has bequeathed 400,000 dollars to the Massachusetts Institute of Technology; and 500,000 dollars to Harvard College, Cambridge.

The *Scientific American* of February 4 reports on the results of the investigation of the late Mr. Keely's laboratory, which seem to show that the mysterious motor phenomena produced by "this nineteenth century thaumaturgist" were due to carefully concealed arrangements for the distribution of compressed air from a three-ton sphere beneath the building. Wonders do sometimes cease.

The 13th meeting of the "Anatomische Gesellschaft" will be held at Tübingen from May 21 to 24.

Professors Laguesse of Lille and Nicolas of Nancy have organised an "Association des anatomistes" analogous to the German Anatomische Gesellschaft. The first meeting was held in Paris in January.

The American Society of Naturalists has elected Professor G. W. Farlow of Harvard as its new president, and Professor T. H. Morgan of Bryn Mawr College as secretary.

The new president of the American Psychological Association is Professor John Dewey of Chicago.

An Anthropological Society has been started at Amsterdam. The president is Dr. C. Winckler; vice-president, Dr. E. Dubois; secretary, Dr. Sasse, fils; treasurer, Dr. C. Kerbert; librarian, Dr. J. E. Grevers.

At a meeting of the Scottish Microscopical Society, on February 17, Dr. Gregg Wilson read two short papers on *Ceratodus*. In the first it was shown that the lung arises in the two-months-old form as a mid-ventral diverticulum of the gut; in the second it was pointed out that the development of the pronephros bears a startlingly close resemblance to that of the newt.

On February 14, at the Royal College of Surgeons, Sir William MacCormac delivered the Hunterian Oration in the presence of the Prince of Wales, who is an honorary F.R.C.P., and a distinguished company. While recognising that Hunter was chiefly and finally a surgeon, he emphasised that his work was in the first instance biological. Our enjoyable contemporary *The Outlook* recalls a classic incident: "Interrupted one day in the midst of the dissection of a

rare and interesting specimen by the message that a patient was waiting in his consultation-room, he at first refused to see him. On second thoughts, however, he threw down his scalpel, and rising, with a weary sigh, exclaimed, 'But I suppose I must go and earn that d——d guinea!'—which is life in an epigram."

At a meeting of the Royal Physical Society of Edinburgh, on February 15, Mr. William Evans submitted a list of the Collembola and Thysanura of the Edinburgh district, drawn up by Mr. G. H. Carpenter, of Dublin, and himself. The list, which was based entirely on specimens collected by Mr. Evans during the past three years, dealt with forty-four species of Collembola and five of Thysanura. Six of the former were additions to the British list. Mr. J. G. Goodchild made a second communication on the genesis of some Scottish minerals, in the course of which he dealt with the changes which had been produced on minerals by the percolation of water from the surface downward. Mr. W. Eagle Clarke read a paper dealing with the recent appearance in Scotland of Macqueen's Bustard, and he also made a communication the subject of which was a Hebridean example of the Lesser Whitethroat.

The Egyptian Government, with the co-operation of the authorities of the British Museum, is about to begin a survey of the Nile, with the object of determining the species of fishes inhabiting its waters. The undertaking has been organised by Dr. John Anderson, F.R.S., who has long been zealously at work on the zoology of Egypt. His proposal for an investigation of the waters of the Nile from Cairo to its origin in Lake Albert met with strong support from Lord Lister, Professor Ray Lankester, and other distinguished men of science. It was also received favourably by Lord Cromer, with the result that the survey has been decided upon. The authorities of the British Museum have, it is understood, promised their assistance, and have also placed at the disposal of the Egyptian Government the services of Dr. Boulenger for the purpose of working out the material obtained by the survey. A number of places along the river are to be selected, at which collections will be brought together and placed in the Museum tanks, which, when full, will be dispatched to London. Mr. Leonard Loat has been appointed superintendent of the survey, and he will act under the direction of Dr Keatinge, chief of the Medical School of Cairo. Mr. Loat is leaving England in a few days to commence operations. The physical characteristics of the river and the river bed are to be carefully noted, and attention given to the habits of the fishes.

Mr. W. W. Skeat, of Cambridge, and formerly of Siam, has started, along with two zoologists and a botanist, on a scientific expedition to the southern regions of Siam, of which relatively little is known.

A report of the work done by the German Deep-Sea Expedition, up to the time of its arrival at Victoria, Cameroons, is given by Dr. G. Schott in the *Annalen der Hydrographie*, Heft 1, 1899.

Mr. John Whitehead has left for another scientific expedition to the Philippines. Mr. Whitehead's previous explorations were confined mainly to the island of Luzon. On this occasion he proposes to visit the southern islands, especially the great island of Mindanao, and should he succeed in penetrating into the mountains very valuable and interesting results are expected. If he is compelled to abandon his journey in Mindanao, he proposes to explore Formosa, Hainau, or the high mountains on the Siamese side of the Malay Peninsula.

Mr. P. G. Ignatof reports in *Globus* (lxxv. No. 3) that the salt lake of Kyzyl-Kak, in West Siberia, which is said not to freeze, has a bright red colour, due to the large number of small crustacea.

Mr. H. J. Elwes has recently returned from the Altai Mountains. One of his chief objects was to visit the head-waters of the Yenisei, almost unknown

even to the Russians—a great valley 300 miles wide by 200 miles long, scarcely inhabited, and practically unexplored. Mr. Elwes has brought home a large collection of butterflies and moths. On the high mountains of the south he secured three specimens of the famous wild sheep, one having a measurement of sixty-two inches round the curve of the horn. Several fine heads were also obtained of the great stag of the Altai. Mr. Elwes also made a good collection of plants from the valley, but unfortunately, owing to an accident when crossing a river, the greater part of that collection was lost.

Dr. Sven Hedin intends to make another journey in Central Asia. He will start from Kashgar, cross the Takla Makan desert by a new route, and pass through Tibet to India.

In the last number of *L'Anthropologie* (December 1898), Marcellin Boule abstracts the discussion on the plateau-flints of S.-E. England that appeared in our pages rather over a year ago. He sums up as follows :—"The problem of plateau-flints then is not elucidated. Perhaps this is because very different objects have been confused under a single name. Shapes like those figured by the English authors are found everywhere that flint exists, in all gravel pits, and even in our garden paths. As for those specimens, if such there be, that really do show the undeniable characters of intentional flaking, we must ask if the deposits whence they come are not analogous to those of our plateaux of the North of France, which are so rich and which differ from the valley deposits in their altitude alone." The defenders of plateau-man must try again.

A considerable area in South-Eastern Minnesota is coloured as Cretaceous on the official geological map. This is confessedly based on scattered masses, supposed to be inliers emerging from beneath a coating of drift. Dr. F. W. Sardeson, of the University of Minnesota, has recently examined these, and concludes that they themselves form a part of the north-western glacial drift, any Cretaceous fossils being *remaniés*. There is one possible exception, an area of half a square mile in Goodhue county ; but even here the component strata are much disturbed, and the mass may be a huge erratic. Dr. Sardeson's results were published in the *Journal of Geology*, vol. vi. pp. 679-691, November 1898.

Falcon Island in the Pacific, near Tonga, has disappeared, after an existence of exactly thirteen years. H.M.S. "Penguin," which recently visited the spot, found that the island had sunk three fathoms below the surface of the water. It was created in the first instance by a volcanic upheaval, and another submarine eruption may replace it on the map. Between Auckland and Tonga the "Penguin" took deep sea soundings, attaining a depth of 4762 fathoms. This is said to constitute a record.

The Geological Society of Australasia has written to the Victorian Secretary for Mines, asking the co-operation of the Government in having a new geological map of Australia prepared as early as possible.

The Geological Museum, Brisbane, was recently robbed of some gold specimens and gems of considerable value. The articles were placed in a safe when the museum was closed, and the caretaker found that the wire connecting the alarm bells securing the glass cases had been cut and that the cases had been opened with keys.

The Queensland Government have appointed their geologist, Mr. Robert L. Jack, to supervise the collections of the exhibits to be sent from Queensland to the forthcoming Greater Britain Exhibition at Earl's Court and to represent the colony there. Mr. Jack reached London early in February.

Mr. L. Boutan makes a report (*Arch. zool. expér.* vi. p. 229), of which we have only seen the commencement, on the progress that has been made in instantaneous submarine photography since his memoir on the subject in 1893.

The following interesting bits of information concerning British New Guinea were given by Sir William Macgregor, the recent Administrator, to a representative of Reuter's Agency :—

"It is not a country in which a man can produce wheat or turnips. It is adapted above all things for growing rubber, and for tea, coffee, tobacco, and cotton. Cotton and tobacco are indigenous. The tobacco is of very fine quality and ought to prove a valuable export. Probably the two principal exports will be rubber and gold. There is a great field for rubber plantation, and there are several kinds of indigenous rubber of high quality. Cocoa-nuts grow luxuriantly all over the colony. Water transport is supplied by many rivers, and we have good anchorages and harbours along the coast-line. Railways are not likely to be very much required on account of our excellent waterways.

"The greater portion of the 400 Europeans in the place are engaged in mining, the remainder devoting their energies to general trading. The produce in which they deal is principally collected by the natives, and includes copra, sandalwood, pearls, and a number of other things. A considerable portion of the natives are employed in trading, and many are engaged by the European merchants.

"The natives are now quite settled over large areas of country, sufficiently so to make agricultural settlement under Europeans quite safe. In the remote districts there are, however, hundreds, and perhaps thousands, of tribes who have never seen or heard of Europeans. Since the great cannibal raid of two years ago, when all the war canoes were captured, cannibalism has been practically unknown in British New Guinea, although there may, of course, be an isolated case here and there.

"The prisons are the best schools for the natives. After the murder of Mr. Green, the magistrate, six natives more or less implicated in the affair were captured and put in prison. After a time they were made warders, and were subsequently given positions as constables among their own tribe, their first duty being to arrest the man who was chiefly responsible for the murder, whom we had previously been unable to find. As a result, the culprit was very soon brought in, and when I left all the ringleaders had been captured. This has been the general principle adopted by the Administration—in fact, it is the only possible one.

"The young people are left to the missions, who conduct their education, but the training of the grown-up people is, as I have pointed out, principally in the hands of the police and the gaols.

"The climate is much better than is generally supposed. During ten years I was only incapacitated from fever for about six days. This form of fever is very amenable to proper treatment, and proper treatment is simple. I believe that New Guinea will eventually be considered a healthy tropical colony. The average temperature for the year at Port Moresby is 82½ deg., the highest reading recorded during four years being 97 deg., and the lowest 65 deg. At an altitude of 4000 ft. to 5000 ft. the climate is very agreeable. One very important fact is that we have no hurricanes."

In view of the extensive use of petroleum products for insecticidal purposes, it is interesting to note Mr. L. O. Howard's evidence (*Scientific American*, Feb. 4) that the maggots of a species of *Psilopa* seem to live comfortably in crude petroleum pools. They breathe by well-protected anal stigmata which are periodically protruded above the surface.

Dr. C. Viguier has invented a tow-net suited for rapid pelagic work, *e.g.* on board a steamship. The points of his invention are the extension of the filtering surface by plaiting the silk, the more rapid passage of water through the apparatus, and an increase of solidity. It is described and figured in the "Notes et Revue" of the *Arch. zool. expér.* vol. vi. pp. vi.-xi.

# Natural Science

A Monthly Review of Scientific Progress

April 1899

## NOTES AND COMMENTS.

### An Ancient Mollusc.

The genus *Pleurotomaria* is the oldest among Gastropod Molluscs, and begins its career in the oldest fossiliferous strata, the Lower Cambrian, with the earliest known Trilobites and Brachiopods.

In geological times its race was abundant, more than 1150 species having been described, and it attained its maximum, about 370 species, in Jurassic days, since which period it has steadily declined in numbers.

The genus was long held to be extinct, but in 1855 the first recent example was obtained from a great depth between the islands of Marie Galante and Dominica in the West Indies by Commandant Beau. Unfortunately this shell appears to have been tenanted by a hermit-crab, so that the animal still remained to be discovered, and it was not till 1871 that the living occupant was obtained by A. Agassiz when on the "Hassler" expedition. Since then other examples of the genus have been obtained from the West Indian region and off Japan, that, with odd specimens of unknown locality, bring the total number of known specimens up to twenty-one belonging to four species. In only six instances, however, has the animal been preserved, and five of these have been procured on American dredging expeditions.

Dr. Dall has published a few details concerning the animals of two, *P. quoyana* and *P. adansoniana* (*Bull. Mus. Comp. Zoöl. Harvard*, vol. xviii. 1889), and another specimen of *P. quoyana* has lately been entrusted by Prof. A. Agassiz to Messrs. Bouvier and H. Fischer to make as thorough examination as its preservation permitted of the soft parts. The result of their investigations now lies before us (*Arch. Zool. expér.*, Sér. iii. tome vi. pp. 115-180, pls. x.-xiii.). The animal in question had been greatly damaged in extraction from the shell; the whole of the spiral visceral-hump, with the gills and a great part of the mantle, were wanting. Under these circumstances it is hardly remarkable that the authors' observations are mainly confined to two points—the radula and the nervous system.

The radula is remarkable for the number and variety of the teeth in each transverse row, and the curvature of these rows. On either side of the median tooth in each row 117 teeth, and the rudiments of

yet another, were observed. They vary from broad, flat, simple plates, without any cusp, to narrow, sickle-shaped teeth, armed with three cusps. At the 50th tooth from the centre a tiny brush of hairs makes its appearance, which in the succeeding teeth is more and more developed, the cusps decreasing at the same time till in the 109th the brush alone surmounts a slender and nearly straight tooth. These brush-teeth seem peculiar to the genus.

The nervous system of *Pleurotomaria* is far too complicated to be treated of in the limits of this notice even in abstract, and it must suffice to say that our authors believe, and give good grounds for their belief, that in *Pleurotomaria* the clue will be found to the relationship of the nervous system of the Amphineura to that of the Gastropoda, the pedal cords of the Rhipidoglossa originating by the fusion of the pedal and pleural cords of the Amphineura, as evinced, amongst other things, by the presence of grooves along the under surface of these "pedal" cords in *Pleurotomaria*.

It is an open secret that a more perfectly preserved animal of *P. beyrichii*, from Japan, is at present being investigated by Mr. Martin F. Woodward, so that further interesting details concerning the genus may be expected.

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## Bats and Black Grouse of Ireland.

The current (February) number of the *Irish Naturalist* contains two papers of more than ordinary interest—the first on the Natural History of Irish Bats, by Dr. Alcock, and the second on the introduction of the Black Grouse, by Mr. Barrett-Hamilton. With regard to bats, the present communication is only the first of a series of papers, and deals chiefly with the general characteristics of the order Chiroptera, with a few notes on the habits of particular species. The detailed history of the Irish forms will follow in due course. In the meantime we may express regret that the author (as we gather from a note on page 30) does not apparently intend to follow modern views of nomenclature. Personally we detest such changes, but as they are made by the advanced zoologists, the only sensible thing to do is to follow the lead. In starting with a misprint (*Atalaphe* for *Atalapha*), the author scarcely does himself justice.

Mr. Hamilton, who has previously contributed interesting information with regard to the introduction of the brown hare into Ireland, accepts the evidence of a fossil bone as entitling the black grouse to be regarded as an indigenous Hibernian bird. He then discusses the records of its alleged early occurrence in the island, and follows on with an account of the numerous later attempts to rehabilitate the species. Unfortunately, these efforts have not, thus far, been attended with the success they merit.

# Bats in Burmese Caves.

THE *Administration Report of the Marine Survey of India* for the official year 1897-98 contains much valuable matter, that contributed by the Surgeon-naturalist, Captain A. R. S. Anderson, being of chief interest to our readers. We extract an account of a visit to some caves at Hpagat, 26 miles up the Salween from Moulmein :—

"These natural caves are hollowed out in the base of an isolated limestone hill, about 250 feet high, rising very precipitously from the river, and appear to be very extensive, but difficult and most unpleasant to explore, owing to the great deposits of offensive bats' dung collected into heaps on the floor by the lessee of the caves. The entrance is about 12 feet high, much ornamented by Buddhistic sculptures, and at an elevation of some 20 feet above the level of the river. As the sun was setting we took our stand on a sand-spit facing the entrance of the caves, and soon saw a pair of falcons leave their perch on the trees on the summit of the cave hill, and restlessly fly to and fro over the river. They were speedily joined by Brahminy and common kites and jungle crows, and the entire flock, to the number of probably 60 to 100 individuals, then flew to the entrance of the caves, close to which they remained wheeling about in mid-air. A few minutes later the bats began to issue in ones and twos, and were at once pursued by the watchful kites and crows, but appeared to have no great difficulty in eluding capture by their rapid and jerky flight, and their pursuers made no very determined or long-sustained efforts to capture them, but soon returned to their vigil over the cave. A minute or two passed, and a sudden rush of wings is heard, and the bats are seen to emerge from the cave in a dense stream which slowly becomes more and more closely packed, continues of about the same density for some ten minutes, and then gradually thins away, till about twenty minutes from the exit of the first bat the last has emerged. The stream of bats when at its maximum is some 10 feet wide by 10 feet deep, and so dense as to closely resemble smoke pouring from a chimney in a gale of wind, a resemblance increased by the slightly sinuous course pursued by the bats as they fly off into the after-glow. Indeed, in the great rush the bats are so crowded together that they frequently upset each other, and fall helplessly into the river below, whence by using their wings as paddles, and flapping over the surface of the water, they struggle ashore only instantly to fall a prey to the expectant crow. When the great rush occurs, the falcons, kites, and crows enter the stream of bats, and flying along, in, and with it, and striking right and left, seize as many bats as they require for food. By merely throwing my walking-stick twice into the stream of bats I obtained six specimens, but after the second throw the bats flew at a greater height than before, and out of easy reach of a stick. All the bats I obtained proved to

be *Nyctinomus plicatus*. After the departure of these small bats a much larger species emerged from the cave in small numbers. It was then, however, too dark to see even to shoot them. During the last twenty years the bats appear to have considerably diminished owing to the depredations of their bird enemies, according to the lessee of the caves, but more probably owing to their continual disturbance by the collectors of the bats' dung."

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## Lack of Discrimination in a Mollusc.

THE Gastropod mollusc *Xenophora* owes its name to its habit of coating the exterior of its shell with various foreign bodies. Some writers have even credited it with considerable power of discrimination in the selection of suitable materials. This idea was criticised in our review of Mr. Cooke's chapters on Mollusca in the "Cambridge Natural History," and we find our comments amply justified by Captain Anderson's observations on *Xenophora pallidula* published in the *Report* we have just quoted from. He writes: "All the examples obtained by me seem to show that the sole selective ability it displays is in rejecting foreign bodies that are too large for it to move easily, its criterion being size and not shape or material. Xenophoras from different stations are seen to be very variously ornamented, but the prevailing ornament on their backs is also that suitable-sized body of which the largest quantity is present in the same haul of the trawl, whether this be dead shells or pieces of stone, etc. At one station off Madras, in the working season of 1894, the trawl was lowered over the steamer track in 107 fathoms and the Xenophoras there obtained were found to be adorned with small pieces of cinders and coal. Here they had merely attached to themselves the commonest convenient-sized objects within their reach quite irrespective of material."

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## Hermit-Crab and Sea-Anemone.

A GOVERNMENT Blue-book such as this *Report of the Marine Survey of India* is so little likely to fall into the hands of naturalists, that we make no apology for giving yet another extract. This relates to a hermit-crab, a species of *Parapagurus*, apparently new to the Indian fauna, and its associate, a colonial sea-anemone named *Epizoanthus*.

"The smallest of these Pagurids inhabited gastropod shells on the backs of which were growing very small colonies of the *Epizoanthus*; but as the Pagurids increase in size while the size of the mollusc shell remains stationary, they can insert an ever-diminishing part of their abdomens in their hosts' shells; and were it not that the *Epizoanthus*

increases in size *pari passu* with the growth of the Pagurid, and grows out as a projecting rim far beyond the mouth of the gastropod, the Pagurid would speedily be shelterless. As the *Epizoanthus* increases in size, the gastropod shell, which at first occupied the centre of and supported the colony, assumes a position near the periphery of the colony and at the same time loses its lime salts so completely that it causes no grating on being cut with a knife. In the largest colonies, which are some four inches in diameter and one inch in thickness, only the very apex of the mollusc may be visible, and that only on carefully hunting for it, so deeply in the substance of the colony is the gastropod imbedded. The hollow space in the *Epizoanthus* colony occupied by the Pagurid is lined by a thin but firm layer of brown chitin, which is easily detachable from the underlying cartilaginous-like body substance of the *Epizoanthus*. The colours of the *Epizoanthus* contrasted strongly with those of its guest; for, while the latter had yellowish-pink claws, orange legs, bluish orange carapace with pale blue spots and an abdomen dirty brown in the male, crimson in the female, the ground colour of the former was madder brown with purple polyps."

We have by no means exhausted the interesting matter contained in Captain Anderson's report, to which we would refer all students of bionomics and of geographical distribution. It is a pity that the price and publishers are nowhere stated on it; but we presume that it is published in the technical sense.

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## The Zola Dossier.

"Je te répète que tout y est," said Dr. Pascal, as he showed Clotilde the famous family papers. And certainly we can echo this remark upon reading the somewhat bewildering pamphlet, "Emile Zola, a Study of his Personality, with Illustrations," which Mr. Arthur MacDonald, the author of "Abnormal Man," "Le Criminel-Type," "Criminology," and other learned books, has just produced. Assuredly "everything" is here,—everything that we do not want. We learn all about Zola's stomach, the size of his ears, and the length of his nose; the secrets of his digestive apparatus are laid bare to us; we learn with emotion that his sense of smell is so good that he knows beforehand what there is for dinner; "he can distinguish tomatoes, chicken, mutton, and different *species* of fish" (the italics are ours); it further appears that the "Bizygomatic diameter of head" is "146 mm.," while as to Zola's eyelids how instructive it is to note: "Palpebral fissure or slit medium; superior left one uncovered."

Against such advantages as these must be set the fact that "the teeth are bad. The alveolar arch is not normal"; while "he is exceedingly sensitive to pain."

Zola suffered much from poverty in his youth, but "when he began to be successful, he was more at ease." Quite so:

> La Palisse eut peu de bien
> Pour soutenir sa naissance;
> Mais il ne manqua di rien,
> Dès qu'il fut dans l'abondance.

There is a certain stateliness in the following splendid marshalling of diseases: "Zola has orbicular contraction, cardiac spasms, thoracic cramps, false angina pectoris, sensory hypcraesthesia, obsessions, and impulsive ideas." With this swan-like song the booklet crackles out, and we arc not sorry. We do not know how much of this precious sort of nonsense is produced annually in the United States, but if by any chance anybody there or in this country is under the impression that this is science, the sooner his mind is disabused of the illusion the better. A study of Zola's personality forsooth, why, it is merely a collection of futile and rather disgusting details about his person,— a dull sort of drivel which is in reality a mingling of the impertinences of the interviewer with the indiscretions of the family physician. We cannot penetrate into the mysterious rccesses of the artist's personality by observing the effect which aperient medicine produces upon him; we may measure with exactitude the length of his ears, and count minutely the number of hairs which grow on the back of his hand, but these things will not teach us wherein lies the magic of his style, nor give us the divining rod by which to discover the hidden springs of his fancy.

## What is Life?

"WHAT is Life?" asks Dr. Gustav Mann (*Trans. Oxford Univ. Junior Scientific Club*, No. 6, Feb. 1899, pp. 99-101), and one seeks in his three lively pages for an answer, which, it is almost needless to say, one does not find. Dr. Mann is evidently unwilling to postulate a "special unexplainable force," but he admits that the origin of organisms meant a new synthesis. "We must have, in addition to thc old laws which govern the inorganic world, new ones which govern and regulate organic existence." This may be a very sound conclusion, but it is couched in strangely mediaeval phraseology for the Oxford University Junior Scientific Club.

"The difference between inorganic and organic existence amounts to this: that the inorganic world has not the power of producing a chain of chemical events, or rather a cycle of events; but that when one chemical affinity is satisfied there ensues rest till a new substance, for which it has a greater affinity, is brought into contact with it; this meeting being one of chance (mathematical probability), or design (chemist). Organic individuals, within physiological limits, are inde-

pendent of chance, as the combination of compounds peculiar to each individual leads to the formation of a new environment consisting of complex carbon compounds, which in their turn, by acting on the world at large, so modify the latter as to make it directly assimilable by the nucleus. The nucleus in its turn forms the organic environment or cell-plasm by which it is kept in existence."

Dr. Mann calls this an "interpretation of life," but it seems to us rather dull. It is a description, and not a very lucid one, of one of the characteristics of the chemical changes observed in organisms, rather than a re-statement in simpler terms, which is the only scientific interpretation we know of. But some of Dr. Mann's paragraphs are more luminous. "In a cell we have a great many comparatively simple compounds, which only collectively form the protoplasm. What constitutes life is the presence of a number of such 'organic' compounds, capable of mutually reacting on one another, and thereby giving rise to new compounds which, because of their origin, have no further chemical interaction, and which therefore will form a comparatively stable mantle round the unstable or active groups which gave rise to them."

Thus the cytoplasm is a new environment created between the chemically active groups (chiefly nuclear) and the world at large. It has the functions—(*a*) of elaborating food, (*b*) of protecting the nucleus from deleterious influences, and (*c*) of attracting food to the cell, or of moving the cell towards food. But what, after all, *is* life?

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## Cycadofilices.

In a paper read before the Royal Society on January 26, Dr. D. H. Scott, F.R.S., gave an account of the structure of a new representative of a group of Palaeozoic plants known as Cycadofilices. The plants included under this head are among the most interesting of all fossil types, as they furnish very important evidence as to the lines of evolution of both ferns and cycads. Dr. Scott has recently had an opportunity of investigating a large number of specimens of a new species of the genus *Medullosa*, discovered by Messrs. Wild and Lomax in the Ganister beds (Lower Coal-Measures) of Lancashire. This first British example of the genus has been named *Medullosa anglica*. It differs in several points from the continental species, and adds some valuable facts to our knowledge of the Medulloseae.

The stem—measuring about 7 cm. in diameter—was thickly clothed with the large leaf-bases of compound fronds, bearing ultimate segments of the form long familiar in the common Palaeozoic genus *Alethopteris;* and branched roots were given off in vertical series between the confluent bases of the leaf-stalks. In habit the plant must have borne a close resemblance to some recent tree-ferns.

The vascular system of the stem consists of three (or locally four) steles, anastomosing and dividing at long intervals. Each stele is made up of a central mass of primary tracheids and conjunctive parenchyma, enclosed by a band of secondary wood and phloem, exhibiting the characteristic structure of recent cycad stems. The stem of *Medullosa anglica* is best described as a polystelic *Heterangium*. The leaf-stalks have the structure of the well-known *Myeloxylon* petioles, which are frequently met with in the calcareous nodules of the Coal-Measures. The course of the leaf-traces is peculiar, and differs from that in any known cycadean plants. The roots, not hitherto known in *Medullosa*, were triarch in structure, and developed a fairly broad zone of secondary wood and phloem.

Without attempting to enumerate the many facts of interest brought out in Dr. Scott's able paper, we may conclude by quoting the author's opinion that while *Medullosa* combines in a striking manner the characters of ferns and cycads, it should not be regarded as having lain "very near the direct line of descent of the latter group."

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## What constitutes Publication?

THIS is always a vexed question. Those who contend that simple distribution by an author is equivalent to publication should note the remarks of Mrs. A. F. Kenyon in the *Victorian Naturalist* for December 1898, vol. xv. p. 99. This lady printed a List of Victorian Marine Mollusca, and posted a copy to various conchologists, including Professor R. Tate, who criticised its misprints in the journal mentioned. Mrs. Kenyon now says: "As the list was not published at the expense of any society, or offered for sale, I fail to see what right your correspondent had to review, it having been sent to him as an act of courtesy." Why on earth any one should go to the expense of printing matter of this kind "for private circulation only" we never could imagine. All the same we agree with Mrs. Kenyon that her list was not published, and should not have been referred to in public print, whether favourably or unfavourably.

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## The Discoverer of Kitchen-Middens.

IN a well-reasoned paper, Dr. William Sörensen of Copenhagen asks and answers the question, "Who was the discoverer of the refuse heaps or 'kitchen-middens' of the Stone Age?" The designation which has had so great a vogue is no doubt due to the celebrated and versatile Johannes Japetus Smith Steenstrup, and he has not uncommonly been credited, not only with having invented the telling name, but also with

having first discovered the unexpected nature of the great shell accumulations. He acted as reporter for the trio who, together or separately, at first unofficially, and then as an official committee, investigated those vast leavings of prehistoric luxury. The three scientific men concerned were Forchhammer, Steenstrup, and Worsaae, and the transaction begins half a century ago. But by a careful comparison of dates, and casual notes in a diary, and the precise wording of reports, Dr. Sörensen has clearly shown that the first and second of those authorities were forestalled in the true explanation by the third. While Forchhammer and Steenstrup were still accounting for the position of the seeming oyster-beds, high above the sea-level, on such hypotheses as the depression of the water or elevation of the land since the molluscs had been deposited, Worsaae had the luck and the wit to find evidence that the phenomenon was indisputably due to the agency, not of inanimate nature, but of man. Steenstrup's own earlier researches had contributed to prove the existence of men in Denmark at a time when the shell-heaps were in process of accumulation, so that he readily endorsed a discovery which he may have thought that Worsaae had taken almost from between his teeth. Fifty years ago it was an audacity to believe in men so very ancient as these oyster-eaters. Now we only think of their audacity in eating so many oysters.

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## The Patagonian Ground-Sloth, Neomylodon.

Zoologists are still much exercised concerning the supposed existing ground-sloth of Patagonia, and the Zoological Society of London on February 21 devoted the greater part of an evening to a discussion of its skin. We were the first, last October and November (vol. xiii. pp. 288, 324-326), to recognise the importance of Dr. Ameghino's announcement of the discovery of this remarkable skin fragment, which he briefly described under the name of *Neomylodon listai*. We also made his privately-printed pamphlet generally accessible by inserting an English translation of it in our November number. Two months ago we announced (vol. xiv. p. 171) that Dr. F. P. Moreno, Director of the La Plata Museum, had brought a piece of this identical skin to England and exhibited it to the Zoological Society of London. We now have pleasure in briefly reporting Dr. Moreno's account of the discovery of the specimen, and Mr. Smith Woodward's detailed description of its characters, which formed the basis of the Zoological Society's discussion on February 21, to which we have referred.

It appears that Dr. Moreno found the piece of skin hanging in a tree at a farm near Consuelo Cove, Last Hope Inlet, in Southern Patagonia. On his expressing interest in the specimen, the owner

took him to a cavern where it was said to have been found, buried in earth, two years previously. Dr. Moreno at once began to make excavations, but did not succeed in recovering anything more except some unimportant bones of rodents. He had therefore to remain contented with the skin which he first noticed, and transmitted this direct to the La Plata Museum. He learned that Dr. Otto Nordenskjöld had already taken a piece of the specimen to Sweden, and that some Chilian officers had carried away other pieces; he also had reason to believe subsequently that Dr. Ameghino had obtained a portion. Dr. Moreno's impression was, that the skin belonged to the extinct ground-sloth, *Mylodon*. Although the specimen had an extremely fresh aspect, and had evidently been removed from the carcase by man, he perceived no difficulty in this supposition; for the Mylodonts were known to have been contemporaneous with man in other parts of Argentina farther north, while in another cave in the same district he had already found a well-preserved mummified human body of an extinct race, which was entirely unknown even to the existing Tehuelche Indians of that region.

At the Zoological Society's meeting, Mr. Smith Woodward began his observations by emphasising the remarkably fresh aspect of the skin. A coating of dried serum was even still preserved on the old cut edges. He would, indeed, have unhesitatingly pronounced the skin to belong to a recent animal killed quite lately, had not Dr. Moreno been able to give so circumstantial an account of the discovery. The specimen was doubtless referable to an Edentate, but it was unique even for a member of that order in having the armour of ossicles confined to the lower half of the dermis, while the covering of hair was implanted in every part of the upper half. The ossicles were very similar in structure to the bony plates of the armadillos; in fact, intermediate between the latter and the known ossicles of *Mylodon*. The hair was also like that of the armadillos, and there was no under-fur. Mr. Woodward thought he could recognise part of the left ear and left cheek at one corner of the specimen, which would imply that the skin belonged to the neck-region; and in this case it was quite large enough for *Mylodon*. The bony armour of this part of *Mylodon*, however, was still unknown; the ossicles of the skin of this great beast had only been definitely described from the lumbar region. Hence the impossibility of deciding between the two rival hypotheses of Drs. Moreno and Ameghino. Mr. Woodward said he could only confirm the opinion that the skin truly belonged to a Mylodont ground-sloth; he could not determine the genus without more evidence.

In the interesting discussion which followed these observations, Professor Ray Lankester expressed his hesitation in accepting the problematical skin as that of an armoured ground-sloth. From the structure of the hair and the ossicles he suspected it might possibly represent an unknown group of armadillos; but he anxiously awaited

more evidence. Professor Seeley and Mr. E. T. Newton favoured the theory of Drs. Moreno and Ameghino and Mr. Woodward. Several speakers alluded to the difficulty of believing that the specimen had been naturally preserved in a cavern since prehistoric times; but Dr. Garson thought the discovery of the ancient human mummy in the same neighbourhood was conclusive proof of the favourable nature of the climate.

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## More about Neomylodon.

THE pieces of the skin of *Neomylodon* collected by Dr. Otto Nordenskjöld have been turned to good account by Dr. Einar Lönnberg, of Upsala, who has just issued a detailed description of them in the report of the Swedish expedition to the south of Patagonia.[1] Dr. Nordenskjöld took his specimen when the piece of skin was about 1·5 metres in length—it is now only half a metre square. He was also first to explore the cave scientifically (in 1896), and there he found the sheath of a claw which might very well belong to a large ground-sloth. Unfortunately, however, there was not a trace of the bones of the animal.

Dr. Lönnberg considers that *Neomylodon* was an animal of the approximate size of a small rhinoceros, or one of the Pampean mylodons; and this leads him to conclude that the living animal stated to have been seen in Patagonia by Lista, and compared to an Indian pangolin, must have been a totally different creature, and not, as Ameghino supposes, identical with *Neomylodon*.

That the latter was a relative of *Mylodon*, and not a cousin of the glyptodonts and armadillos, is considered by Dr. Lönnberg to be practically certain. Not only are the dermal ossicles akin to, although apparently to a certain extent different from, those of *Mylodon*, but the claw is of a mylodont, as distinct from a glyptodont type. With regard to the hair, Dr. Lönnberg considers that in his specimen the outer sheath has in each case been removed, and that only the core remains. And he is thus led to believe that a very close analogy exists between the hair of the Patagonian animal and that of the sloths. If, as he is inclined to think probable, some of the hairs of the former were coated with an alga, there would seem undoubted evidence of bradypodine affinity in this respect. But before the theory that the hairs in their present condition are nothing more than cores be definitely accepted, it would be wise to wait and see what Mr. Smith Woodward has to say on the subject.

Apparently Dr. Lönnberg is of opinion that *Neomylodon* is generic-

[1] "On some Remains of *Neomylodon listai*, Ameghino, brought home by the Swedish Expedition to Tierra del Fuego, 1896," *Svenska Exped. Magellansländ*, ii. pp. 149-170, pls. xii.-xiv. Stockholm, 1899.

ally distinct from *Mylodon*, and also that it has now probably ceased to exist. After mentioning that it might possibly have escaped the notice of the scattered white population of Patagonia, he adds that "it is absolutely impossible to think that this animal, if it was still among living beings, could have eluded the sharp eyes of the native Indians. Even if it had exclusively nocturnal habits, and hid itself during the daytime in the most desolate places, the hunting Indians must have come across it now and then, and they must certainly have observed its tracks, its traces where it had broken off branches and twigs when feeding, its scratchings or diggings in the earth, its excrements, etc."

In all the above there is doubtless much of truth; but, on the other hand, it must be remembered that a very large proportion of the naturalists who saw Dr. Moreno's specimen were inclined to regard it as of very recent origin indeed. And if the creature has been alive a few years ago, there is a strong probability that it still survives. With regard to its generic distinction from the mylodonts of the Pampean, it may be mentioned that many of these lived on to a comparatively late epoch, and also that most, if not all, of the modern genera of armadillos were already in existence during the Pampean period. This being so, it does seem, *primâ facie*, somewhat strange that *Neomylodon* should be entirely of post-Pampean origin, more especially if it is now either extinct or on the verge of becoming so.

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## Supposed Mesozoic Mammals from Patagonia.

THE preliminary results of Dr. Santiago Roth's discovery of so-called Mesozoic mammals in Patagonia, to which we referred some time ago (vol. xiii. p. 439), are rather disappointing. They appear in the *Revista* of the La Plata Museum (vol. ix. pp. 381-388), and a detailed memoir is promised later in the *Anales* of the same museum. The remains were found by Dr. Roth, associated with bones of Dinosaurian reptiles, in no less than three distinct formations. In one place, moreover, a marine band was intercalated between the mammal-bearing deposits, and this contained casts of shells which are said to occur in the undoubted Cretaceous of Brazil. The mammals themselves, however, are totally different from those primitive types which we have hitherto known to be of Mesozoic age. If they were found in the northern hemisphere, indeed, and if their exact date were unknown, we should not be surprised if mammalogists claimed them to be late Tertiary. Nine new genera are founded, all apparently most closely related to the aberrant hoofed animals which are so characteristic of the Tertiary formations of South America. It seems to us more likely that Dinosaurs survived in the Tertiary fauna of the South American continent, than that highly specialised Ungulata were already

differentiated there in the midst of the Mesozoic fauna; but whatever be the geological result of these discoveries, we await Dr. Roth's completed memoir with the greatest interest.

## A Fine Taste in Binding.

In Mr. Thomson's review of Professor Ewart's "Penycuik Experiments" (*Natural Science*, xiv. pp. 203-212) no notice was taken of a feature which should not have escaped a naturalist's eye, we mean the "external characters." It is with these that a biological description should begin, and in this case they are striking enough. We believe that the quaint work called *The Evergreen*, also an Edinburgh product, had its spring number at least clothed in lamb's skin, which was suggestive enough of the gentle bleatings within. It is strange therefore that one of the authors of *The Evergreen* should have failed to remark that "The Penycuik Experiments" are clothed "in zebra." We would congratulate the author and the publishers on this elegant binding in the hope that others will follow their example. How much it would add to the grace of our bookshelves, how much it would lessen the labour of finding a book, if each volume were so to speak indexed by its binding. How ineffably better than any decimal notation if biologists would in their binding rehabilitate the "doctrine of signatures." We read that "Matopo" became strangely excited when he saw a zebra skin; we should like to know what he did when he saw the book.

## The True Function of the Thymus.

Dr. John Beard is to be congratulated on having arrived at the solution of a problem which he has had before him for years,—the function of the thymus. Since Kölliker discovered its origin in mammals from the epithelium of a gill-pouch, and stated that the original epithelial cells gave rise to lymph cells or leucocytes, two views have been held regarding this puzzling organ. "On the one hand, Stieda and His have maintained that the leucocytes which always form integral parts of the thymus soon after its first origin have migrated thither from the exterior, possibly from the mesoblast. In this conclusion they have been supported by the researches of Dohrn, Gulland, and Maurer, and by almost every text-book of embryology and comparative anatomy published since 1879. On the other hand, Kölliker has stoutly maintained his original position, and the results of his investigations have been emphatically confirmed by Prenant, Oscar Schultze, and Beard." So far the historical aspect.

But Beard has now shown (*Lancet*, January 21, 1899) that in the

smooth skate (*Raia batis*) leucocytes appear at a time when the spleen has no existence, when there is no rectal gland nor lymphoid structures of any sort. The leucocytes are then present in the epithelial cells of the thymus primordia. Thence they soon begin to migrate, emerging in crowds which cause larger or smaller "breaks" at various places on the contour of the thymus. As the author puts it:—"It is Kölliker's great service to have shown that leucocytes arise in the thymus from its original epithelial cells; to Gulland's researches we owe the result that the first leucocytes are found in the mesoblast in the neighbourhood of the thymus; and, finally, it has fallen to my lot to show that the first leucocytes arise in the thymus from its epithelial cells, and that thus it is the parent source of the leucocytes of the body." We are not clear as to the logic of the "thus" in the last sentence, but the discovery is a triumph, and we eagerly look for a full paper with illustrative figures.

## Antherozoids in Gymnosperms.

THE long-expected account of antherozoid formation in *Cycas revoluta* has at length appeared in a form which renders the extremely valuable results obtained by Mr. S. Ikeno available in all their details to the ordinary European reader (*Jahrb. f. wiss. Bot.* Bd. xxxii. 1898). Several notices which have from time to time appeared in various publications have already made us acquainted with the general outline of the research, but we have now before us for the first time a complete account of the process of fertilisation in *Cycas* as it takes place under normal conditions in the natural habitat of the plant.

The development of the archegonium differs but little from that in other Gymnosperms, except as regards the enormous size, 4 mm. × 1 mm., reached by the adult organ. The most remarkable results obtained are, however, those relating to the germination of the pollen grain, a process for the completion of which a period of three months is required.

The greater part of the ripe spore is composed of a large round "embryonal" cell, accompanied by two much smaller "germinal" cells placed one above the other close against the inner side of the wall. The pollen tube arises in the usual way from a thin area in the wall of the embryonal cell, the nucleus of which accompanies the apex of the tube as it bores its way into the nucellar tissue, where it branches and apparently soon becomes quiescent. Meanwhile the two germinal cells have become spherical, at the same time increasing considerably in size, especially the inner one, the nucleus of which divides in a direction at right angles to the axis of the pollen tube, without however any formation of a cell wall. One of the resulting daughter nuclei commences to grow rapidly while the other remains of small size, and is ultimately crushed against the wall by its more successful sister.

A similar occurrence has been recorded by Strasburger (*Histol. Beitr.* Heft iv.) in certain Coniferae, but in this case the division is parallel to the axis of the pollen tube, and a distinct wall is formed. With this difference, however, the processes are identical, and the crushed nucleus of Cycads must be looked upon as homologous with Strasburger's "stalk" cell, while its sister nucleus corresponds to that of his "body" cell.

At this stage the embryonal nucleus begins to move back from the apex of the pollen tube, and eventually comes in contact with the body cell, now considerably enlarged, elliptical in outline, and filled with dense protoplasm. The exine covered remains of the pollen grain thus contain, besides the body cell, the outer germinal cell, the nucleus of the embryonal cell, and that of the stalk cell, all of which presently swell up and disappear, being apparently absorbed by the body cell, which continues to increase in size and eventually divides, the daughter cells remaining for some time connected by a strand of faintly staining cytoplasm. The plano-convex cells which result from this division lie with their flat surfaces in contact, and become the antherozoids. Each consists of a large nucleus enveloped in a comparatively thin sheath of cytoplasm, in which not far from the centre of the convex side there appears a short band, the origin of which is tentatively ascribed by the author to the centrosome. This band, at first composed of granules which soon fuse to form a filament, comes in contact with a beak-like process thrown out by the nucleus, increases rapidly in length, and finally describes four or five complete spiral convolutions directly beneath the convex surface of the antherozoid. Its outer face is clothed with cilia, which, formed at first within the cell, ultimately project beyond it into a spiral groove running parallel to the band, and indenting alike cytoplasm and nucleus. The antherozoids when ripe escape from the pollen tube into the fluid-filled space within the membranous remains of the nucellus; here they swim to the neck of the archegonium and thus enter the egg cell, where the nucleus slips out of its cytoplasmic sheath, leaving it to disappear in the upper part of the cell while it moves down to the centre to fuse with the nucleus of the egg.

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## The Broad Path.

EVERY one has no doubt heard of the ingenuous youth who defined Grimm's Law as the law whereby any consonant could be replaced by any other, but we have sometimes wondered whether a definition of biology on similar lines might not prove acceptable to some of its students. As thus—Biology is the science which proves that everything is in process of being converted into something else, and that this has an important bearing on human life. We recommend the definition to the notice of the versatile author of "Cycling: its Effect on the Future of

the Human Race" (*Medical Magazine*, 1899, vol. viii. pp. 128-135). We admit that it is defective in that its phraseology is simple, and does not involve those "exact distinctive terms" without which "clearness of understanding is impossible," but our only apology is that the variety of biology to which we have been accustomed does not enable its devotees to attain the clearness gained by describing man as a "degenerating, quadrupedally-ancestored biped." We are afraid also that our biological training did not include the study of the "successive stages in the progress of the human baby towards the attainment of bipedal progression," in spite of the fact that a "knowledge of the outline of such a history is part of an ordinary scientific equipment." We regret these deficiencies the more because we think that "science" like that of Mr. Buckman's article—which, of course, we take as a *jeu d'esprit*—must have many advantages. When one gets habituated to the harmonious multiplication of adjectives, and learns to indulge in bipedal locomotion instead of taking a walk, the broad sweep of the generalisations and the easy superiority to petty detail must have a wonderful fascination, and besides it must save trouble. Biologists of the old-fashioned type have, we believe, spent laborious days in investigating the structure of animal parasites, and yet how needless was their labour! Could they but reach the true light, they would realise the simplicity of nature, would understand that parasites have "reverted to ancestral conditions," where the "body of the organism is nourished by endosmotic action." Similarly, physiologists need no longer toil and experiment, for their problems can be easily solved. Would they learn the cause of the symptoms of alcohol poisoning? Obviously these can be readily explained as a "return towards the quadrupedal stage." Would they as physicians know the cause of baldness in man? Again what is it but reversion? Surely the beauty and simplicity of this method of reasoning must be apparent to all. It is true that it leads to somewhat revolutionary results; thus we learn that the whale is an "instance of return from a higher to a lower state," for it is a "mammal which has gone back to the fish-like form," but we must be prepared to sacrifice a few prejudices to a method which yields such a copious amount of information on so many diverse topics. Does it not teach us that one of the great advantages of cycling is that "the weight is rolled along," while "in bipedal locomotion the body is reared up on end" and "carried, in a most disadvantageous manner, alternately on one limb," and who would not sacrifice much to obtain so priceless a bit of information? Let whosoever will therefore roll with our author down the broad path, regardless of the stubborn few who attempt to bipedally progress up the narrow one. We have entered this protest against science made easy, but surely the sense of humour is not so dead amongst us that it will be supposed that we mean more than that Mr. Buckman has lapsed in his article just as Homer may have nodded.

# ORIGINAL COMMUNICATIONS.

## The Development of Rivers; and particularly the Genesis of the Severn.

By S. S. Buckman, F.G.S.

I recognise the difficulty of the task. I do not pretend to be able to perform it adequately; but I have the hope of placing the subject on what may be a somewhat surer basis for future work.

I. *Introduction.*—A few words as to the evolution of my ideas, showing how greatly I am indebted to other people, may not be amiss. In some topographical articles (1), and in a communication to the Geological Society (2) in reference to some valleys now holding tributaries of the Severn, I pointed out that their upper parts could only have been excavated by streams which were tributaries of the Thames; that therefore there must have been high ground—lost Cotteswold Hills, in fact—to the north and west of the present escarpment; and that the Thames must have been a larger and longer river than now. I said that the diversion of a west-to-east stream, tributary to the Thames, must have been brought about by the working back of a Severn tributary—the one which now forms the Chelt; and that in course of time the present Chelt must serve the same trick on another Thames tributary, the Coln, because it could give so much quicker fall.

But in extending the Cotteswold Hills farther to the west, I certainly had no idea of filling the Severn valley completely with Jurassic rocks, and supposing the non-existence of the Severn itself. However, when I read the paper by Osborne White (3) citing the opinion of Professor W. M. Davis (4), that, in effect, the Severn valley was filled with Secondary strata, and that the Thames streams originally headed back in Wales, what was till then obscure became suddenly clear: the phenomena of the Severn and its tributaries seemed easily understandable. And the more the idea is worked out the more does the evidence in its favour seem to accumulate.

I ought perhaps to make this somewhat clearer. Of course it has always been recognised that the Severn valley was originally filled up

with Jurassic rocks and buried beneath Cretaceous strata; but it had not been understood that the Thames streams flowed across this area for a long time before the Severn had existence. It seemed to be supposed that the Severn and the Thames were contemporaneous, that the Severn cut a channel between Palaeozoic and Mesozoic rocks, and so initiated the Mesozoic escarpments; that the Cotteswold escarpment extended farther west to nearer the present position of the Severn; and has only been worn back therefrom by the further cutting done by the Severn.

Yet I afterwards learnt that some dozen years before Professor Davis put forth his views, Dr. T. S. Ellis (5), of Gloucester, had imagined the possibility of rivers flowing across a non-existent Severn valley. He says, "If we could have it admitted that the trend of the country was originally to the east before the present [Severn] valley was formed at all, following a slope as the Cotteswolds now dip; then one could imagine a stream flowing through the gap between Malvern and May Hills along the line of the Leadon and escaping over the Cotteswolds at Witcombe, so first marking out one of the west-to-east streams of which apparent signs remain."

To his paper he appends a map showing the application in three cases:—A. The Severn to Tewkesbury flowing up the valley of the Swillgate to Cheltenham, and up the Chelt valley to the Coln at Andoversford; B. The Leadon flowing past Gloucester and over the Cotteswolds near Birdlip, and so into the Thames system; C. A river from west of May Hill flowing up the Stroud valley along the course of the Frome.

In his arguments for his theory from the physical features of the country he makes another good point. "The peculiar course of the tributaries of the Severn in our district seems to have a bearing on my suggestion that the first streams flowed across the line of our valley. It is remarkable that nearly all those on the left bank flow towards the river in a direction against that of the Severn itself. . . . Can these tributaries on the left side be occupying channels originally marked out by streams flowing in the opposite direction?"

Lately I have had the great advantage of accompanying Professor Davis to some of our Cotteswold streams, and he has urged me to give an account of the Severn.

This introduction is rather long; but I wish to show my indebtedness to others. I feel that the ideas to be set forward are not mine in any great degree. And, in fact, I know not how much is from myself, or how much has been suggested by the written or verbal communications of others.

II. *General Considerations.*—At the first initiation of the drainage of an area the direction of streams is determined by the dip. Such streams are termed *consequents* by Professor Davis (4). Now it is necessary to consider what would happen in a tilted area composed of rocks of varied power in resisting denudation, and different porosity.

For the purpose it may be said stronger and weaker rocks, also pervious and impervious.

The stream cuts down its beds so that different rocks outcrop in the stream-bed, and are exposed on its flanks. Weak rocks are denuded faster to make combes, impervious rocks send forth springs. The springs and combes become side-streams and valleys. The side-streams are *subsequents*. Now consequents cannot get any better off than at first: they get all the drainage and cannot get more. But subsequents become better off the more they work back. The consequents are dip-streams, they are best off when they start. The subsequents are strike-streams —they are worst off at starting, and best off at finish.

A strike stream has a dip side and an antidip side. It obtains all the drainage of the dip side by the former streams. If it can give the water a quicker fall to a lower level in a shorter distance,[1] it will start streams on the antidip side. Antidip streams are *obsequents*. Like subsequents they become better off the more they can work back. They are really stronger than subsequents, and can in time obtain practically all the drainage of a given area.

In the case of an anticline a consequent could grow by developing obsequents beyond the axis. The Salisbury Avon seems to be doing this in connection with the Pewsey anticline, taking the water and destroying the south drainage area of the Kennet. Artesian action too is possible —it may obtain water which falls far to the north of the Kennet.

In a drainage area which had not suffered previous denudation subsequent streams would be started along anticlinal axes, because there the impervious rock, and hence the water-level of the upper area would first be reached.

In an area which had suffered previous denudation so that different rocks outcrop, the same would apply where there was a wide expanse of a rock of uniform composition. The head-waters of the Salisbury Avon in the Pewsey valley, and the growth of the original Sevenhampton branch of the Coln in the Charlton Abbots valley, are examples. In part this may explain the preservation of synclinal areas.

But in such a drainage area, where stronger and weaker rocks outcrop not far apart, the subsequent streams will be developed along the strike of the weaker rocks. The Ock, and the Thame, and the Thames above Oxford, are examples. So, in fact, are rivers which drain along the strike of New Red Sandstone, Lias, Oxford Clay, and so on.

But this is a digression. What I want to point out is this, there are started, first, dip-streams, secondly, strike-streams, and thirdly, antidip-streams. Where would their position be? Naturally in the lowest ground—the valleys of the beheaded dip-streams or consequents.

This seems to be an important matter to consider in drainage restoration. If obsequents tend to occupy and work back in valleys of

[1] This is important: it is not necessarily a shorter route to the sea, but an immediate quicker fall.

consequents, there would be established a principle of permanence of river valleys, making drainage restoration a certainty. The valley would remain a valley even though denudation had removed its sides far below the original floor of the consequent stream. There is Dr. Ellis's suggestion with regard to the left or antidip tributaries of the Severn; as it would be put now, they are obsequents which occupy and have deepened valleys of consequents, or Thames streams. Diagrammatically the history has been this:—First, consequents flowing N.W. to S.E. (Fig. 1, A), secondly, consequents cut into by a subsequent, and obsequents started up the consequent valleys (Fig. 1, B). Then the obsequents proceed farther up the consequent valleys, their

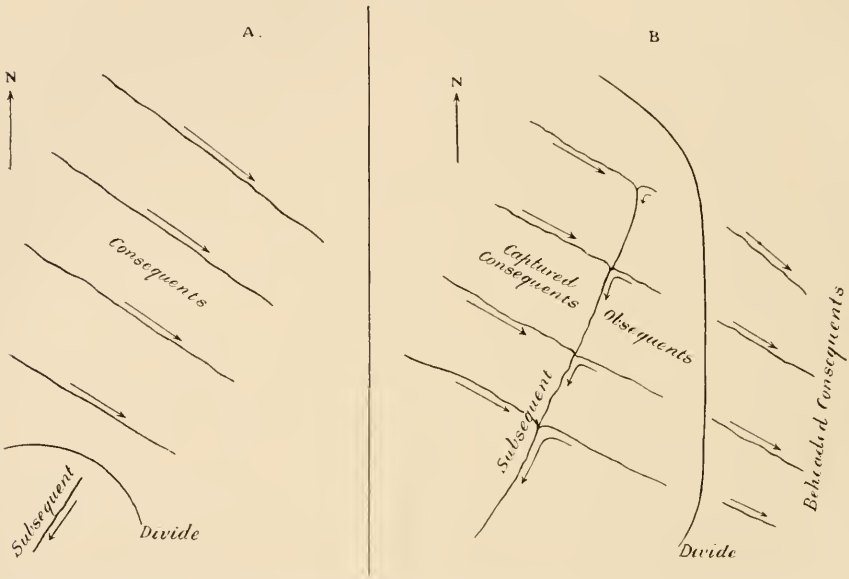


FIG. 1.—Theoretical diagrams illustrating (A) consequent streams threatened by a subsequent, (B) consequent streams captured by subsequent, and initiation of obsequents.

relative lengths according with the successive dates of capture of the consequents. Naturally, then, the obsequents should have a direction S.E. to N.W., and they have. See the sketch map (Fig. 2), which illustrates the actual result, closely in accordance, it will be seen, with the theoretical diagram (Fig. 1, B). This matter is very important; and it follows from considerations of these cases that the beheaded consequent and the growing obsequent should be found occupying opposite sides of the divide with in many cases a continuous valley (Fig. 3).

Therefore in such valleys, which would form passes, the obsequent and consequent streams should be opposite to one another.

From many actual cases I select a rather striking example:—Two cases are shown in Fig. 4, the Isborne and the Coln, in a deep pass: a branch of the Windrush and a branch of the Isborne—the

divide in a slight depression. The Coln had formerly taken off the head-waters of this Windrush branch.

Professor Davis (7) gives an example :—A branch of the Meuse and a branch of the Toul (Moselle) opposite.

This brings me to another matter—passes and depressions often produce the feature of breached escarpments. What is the significance of breached escarpments, and what do they point to in the way of river restoration? To understand them it is necessary to classify them, bearing in mind that mainly they represent beheaded valleys. According to the history of the valleys they have to be classified. There are :—

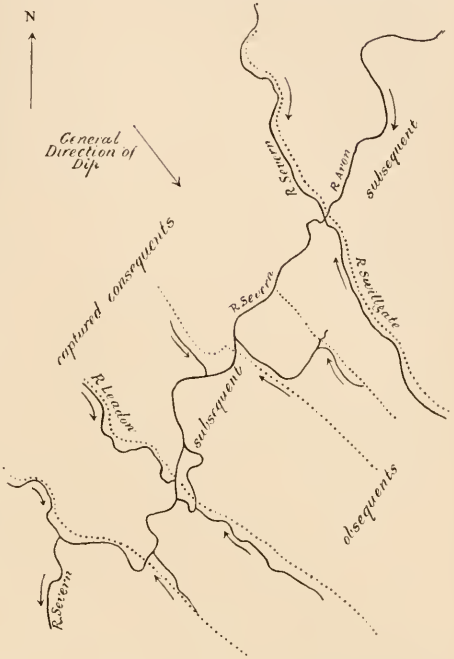


FIG. 2.—Sketch map of the present arrangement of the Severn and its tributaries near Gloucester. The dotted lines indicate the supposed courses of the original consequents. From the Ordnance Survey, Scale, 1″=4 miles.

I. Valleys opening out headward on a plain. A stream drains the plain, and perhaps more beyond; and it flows down the valley with the dip. Examples: — The Goring gorge; the breach in the Chalk escarpment at Upavon, where the Salisbury Avon enters, draining the Vale of Pewsey.

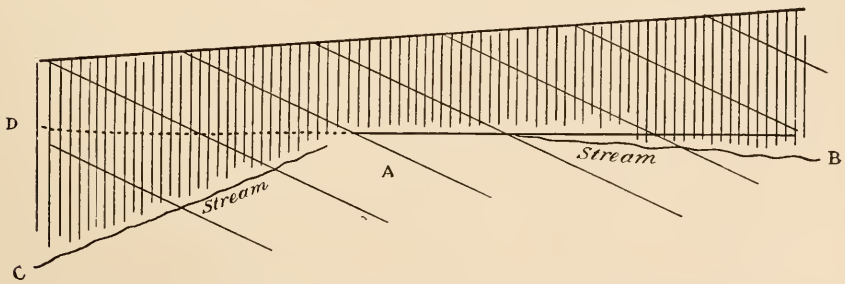


FIG. 3.—Section of valley crossed by divide at A. B is beheaded consequent stream, C is the obsequent stream. D is the original level of the floor of the formerly extended valley of the consequent. This really shows what I have termed a duplicate valley (6). Above D B is one (the original valley); D A C is the second valley. The inclined lines show dip of strata (exaggerated).

II. The valley is similar, but no stream flows through it. If it holds a stream at all it is only in the lower part. Example :—The

Ogbourne gorge, in the Chalk to the south of Swindon towards Marlborough, along which runs the Roman road from Cirencester to Winchester.

III. The valley contains a divide from which the floor slopes both ways: it holds two streams—one flowing with the dip and one against (Fig. 3). Examples:—The pass from Cheltenham to Andoversford; the pass from Winchcomb to Andoversford; the Moreton valley.

IV. The valley contains an antidip stream only. Example:—The Stroud valley.

V. The valley opens headward high above the plain; it only contains a stream in its lower part, if at all. Examples:—More or less

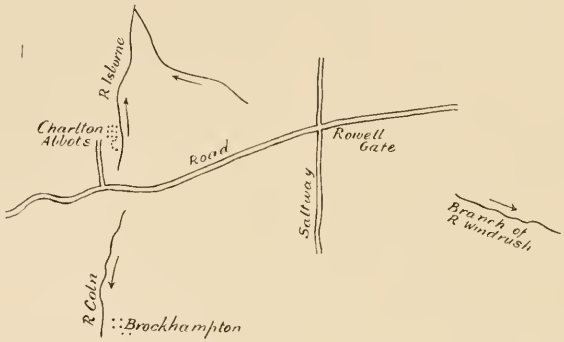


FIG. 4.—Opposition of obsequent and subsequent, and obsequent and consequent, streams near Winchcomb. Scale 1″=1 mile.

dry valleys extending back from the Cotteswold escarpment—a valley leading from above the Vale of Pewsey to the Kennet, in which runs a road from Marlborough to Alton Priors.

To consider the development of these valleys and the escarpment breaches to which they give rise. I. This is due to the cutting down by a consequent stream in the ordinary course of events. II. is due to the same cause; but there is a later phase: the original excavating stream has been tapped and diverted before reaching the gorge. III. This is a further development of II. An obsequent stream has been started, which excavates the original valley-floor of the consequent stream, farther and farther back. This type is found in all stages of development up to IV., in which the original consequent stream has been nearly driven out of its valley, and the valley-floor has been trenched nearly to the mouth of the original valley.

The first type of valley holds a through stream now. The others are considered to have held through streams formerly. Wherefore when they are recognised, such through streams may be given them in river restoration, and according to the relative depths to which the cutting of the original valley-floor has been carried out, so may the further stage of river development be surmised—whether a stream captured its neighbour before it was captured itself, and so forth.

The escarpment breaches connected with these valleys must be separated from those connected with the type of V., which indicate not a through stream but merely local traffic. Their history has been this: when the escarpment stood farther forward there was sufficient collecting ground for the rainfall: a spring could issue at the back of the escarpment and its stream formed a valley; the escarpment has been worn back; the collecting ground has gone; the spring has disappeared; the escarpment has been worn back farther; the head of the valley with the place where the spring once rose has been taken off. The head of the valley forms a notch or breach in the present escarpment.

In drainage restoration a through stream with a distant source must not be accorded to valleys of the type of V., breaching escarpments, only a short local stream must be given.

Among other numerous phenomena of rivers and valleys one further feature must be noticed now, as it is important in drainage restoration. It presents itself as a sharp turn in the course of a stream; not a local twist, but an entire change of the general direction.

When a subsequent or obsequent stream captures a consequent stream an abrupt angle is generally formed at the junction. This angle is called the "elbow of capture" by Professor Davis.

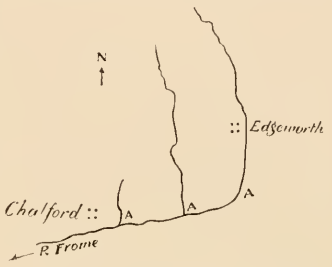


FIG. 5.—Elbows of capture near Stroud.

Fig. 5 shows elbows of successive capture (A, A) of Thames consequents by the obsequent Frome; Fig. 6 illustrates the restoration of the drainage as it was before the captures had been effected. And at the base of the diagram, below Ashton Keynes, is seen the elbow of capture (A) of the there subsequent Thames having taken the Churn. It was the continuation of the Churn which cut the Ogbourne Valley to Marlborough. It was the capture of the Churn by the Thames which has caused that valley to be different from the Goring gorge.

The elbow of capture must be distinguished from a right-angle bend made by the joining of a tributary subsequent to a consequent stream; and especially from the bend which may be at the head of a consequent stream which has been shortened by the successful growth of an obsequent, so that the subsequent stream becomes the longer. The Upper Kennet, where it turns west to the south of Avebury, is a case in point.

It has been necessary to put forward these general considerations, before attention could be devoted to their particular application. Yet again, before they can be applied to the history of the Severn, the development of the Thames must be noticed.

III. *Development of the Thames.*—As seen at the present day the

area of secondary rocks drained by the Thames appears to have been carved out of an original plain which cut across the edges of the strata. It would be a plain of diverse stratal composition.

But when the area was first upraised, there must have been a plain of uniform stratal composition. For short these two plains may be called "a uniform plain" and "an outcrop plain." The outcrop plain has been formed by the removal of the superincumbent strata. But how?

The usual idea is marine denudation. Professor W. M. Davis argues against this (4). He considers the outcrop plain to have been formed from the uniform plain by subaerial denudation—that the

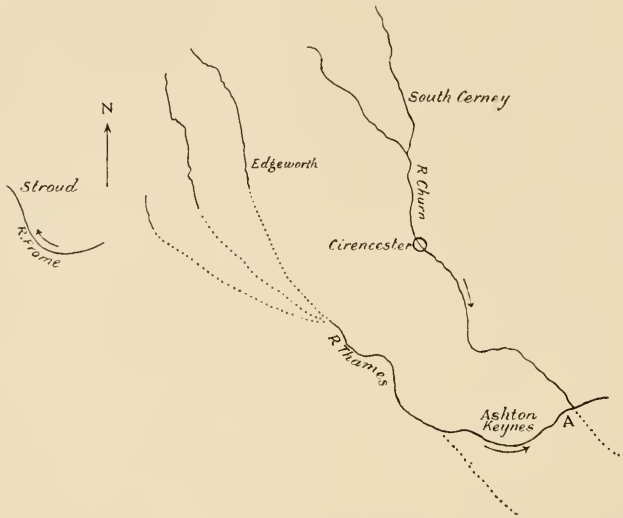


FIG. 6.—Restoration of Thames drainage before capture by the Frome. Elbow of capture of Churn by Upper Thames at A.

rivers cut the country down to a low level: he calls this the first cycle of denudation. The low level plain was subsequently upraised to become a new high level plateau. It is about 700 feet in the Chilterns and about 1000 feet in the Cotteswolds. After the upraising the rivers commenced to cut down afresh—it was the second cycle of denudation.

To me the difficulty seems to be this. Before an area could be worn by river-denudation to a low level of little relief, the third phase of river-development would have reached its possible maximum: a most important system of obsequent streams would have been developed, and they should be as long or even longer than the beheaded consequent streams.

These important obsequent streams are not found. Even at the present day the obsequent streams are not nearly so long as they may be expected to become in order to establish that drainage equilibrium

which must be necessary to wear a given area down nearly to base level.

Drainage equilibrium would be established when the distance from the divide to the sea is the same in either direction. That would be brought about by the growth of obsequent streams.

It seems necessary to suppose that drainage equilibrium would be established before a given area could be worn down to base level.

I confess that it is easiest to start the Thames drainage afresh on an outcrop plain representing a base-level of erosion—a line drawn from about 1300 feet at the Malverns to about 700 on the Chilterns. Starting afresh, however, means marine denudation.

In this plain the Chalk would extend to about Witney, the Severn valley would be filled with Jurassic strata, the Inferior Oolite extending about 3 miles to the west, and the Lias to the Malverns.

An outcrop plain, produced by subaerial denudation, might have been formed in Eocene or Miocene times; but by marine erosion it only seems possible in early Pliocene. That does not seem to give sufficient time. All but the latest details of the Severn valleys are Pre-Pleistocene. However, suppose an outcrop plain, and consider the drainage.

The plain has a general south-east dip, so far as the main area of England and part of Wales is concerned. But this plain seems to have been bounded on the south by a more or less continuous and irregular line of hills—the anticlines of the Mendips, of the vales of Pewsey and Kingsclere, and of the Weald. The consequence was that the drainage of what may be called the great central plain was towards the south-east. But at the southern end of the plain it was checked by the anticlines; from their north side came streams draining northwards, and hence the drainage was given an easterly turn at the south of the plain. Fig. 7 illustrates the position of the supposed lines of original drainage and the positions of the anticlines.

The initiated streams are all consequents; but as they cut down their valleys subsequent streams would be started.

As Professor W. M. Davis points out, the Ock and the Thame are both subsequent streams. So is the Thames from beyond its junction with the Churn to its junction with the Evenlode. This Berkshire Thames was evidently developed before the Ock, by a subsequent growth working west from the Evenlode; it successfully cut into and diverted all the Cotswold streams. Evidently the Windrush once joined the Thames direct somewhere near Abingdon; but the Evenlode, perhaps helped by the Cherwell, had so successfully lowered the country around Oxford that curiously enough it was able to divert the Windrush to a more immediate lower level though giving it a slightly longer course. This was done by the initiating Berkshire Thames subsequent.

The Evenlode was a very large river, draining by one branch the

north Welsh mountains, by the other the west side of the Pennine range.

East of it was a south-easterly extension of the Dove. The Evenlode soon captured this by sending out the Cherwell as a subsequent stream; and on the other side it captured all the Cotteswold streams.

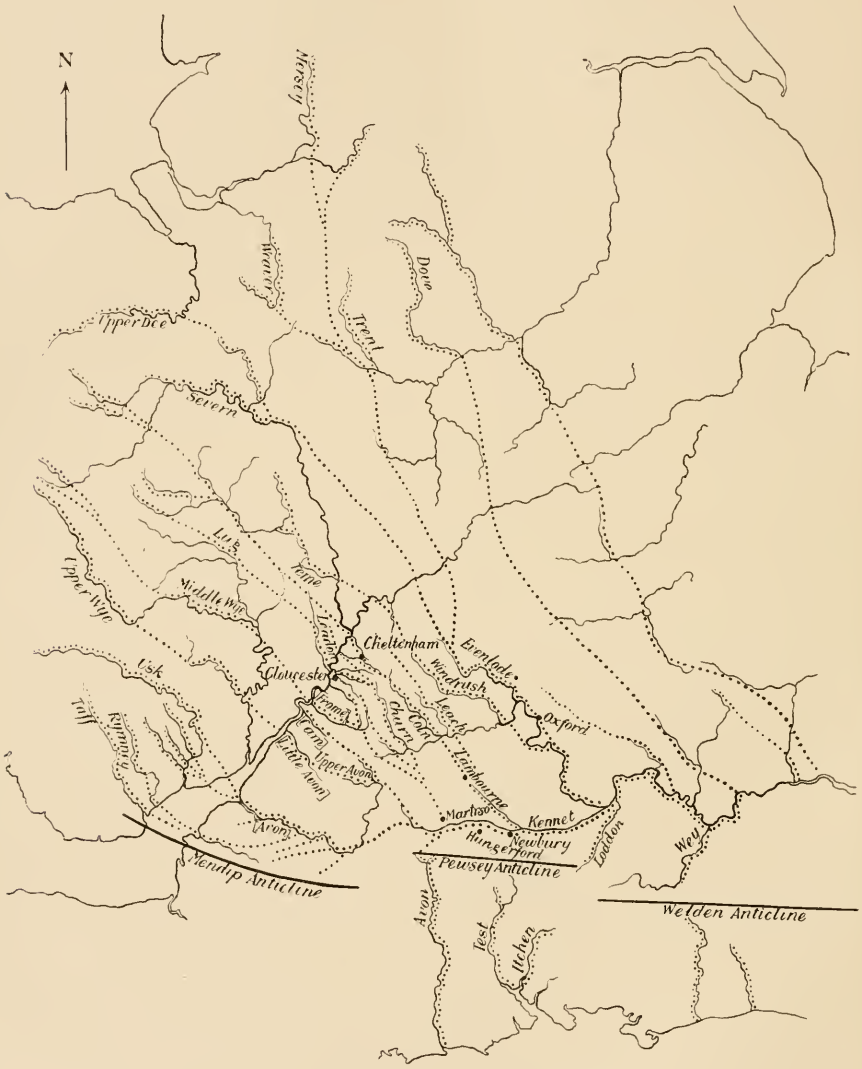


FIG. 7.—Original Consequent Streams.

The Kennet was the other important river. It originally drained Mid and some of South Wales. But the Evenlode robbed it of the Mid Wales water by cutting off the Cotteswold streams. So the Evenlode—including the Thames from Oxford to Reading—was the bigger at starting, and grew bigger in consequence, by robbing the

Kennet. And it is the bigger now; for the Kennet system was the first to be attacked from outside.

But before detailing the incidents of that attack it may be remarked that, on the evidence of the Cotteswold breached escarpment, the Mid Wales or ultra-Severn drainage had been collected into two main channels—the rivers which cut the upper valley at Stroud and at Cheltenham. The latter, the original Coln, seems to have been the most important; by developing subsequent streams it beheaded the Windrush on the north at Winchcomb,[1] and the Churn on the south at Gloucester, thus gaining parts of the Teme, Lug, and Leadon.

The Middle Wye-[Frome][2] does not seem to have made any important captures; but it is surmisable that it gained the Upper Wye, while the Middle Wye and the original Coln lost their head-waters to the Upper Severn extension of the Evenlode. Thus, in what may be called the second phase of the Thames system, the west and north-west drainage had been collected into five principal channels just east of the present Severn—(1) The valley between Dundry and the Mendips; (2) the valley north of Dundry; (3) the Stroud valley; (4) the Cheltenham valley; and (5) the Moreton valley. Of the streams in these valleys the first two joined to make the Kennet, and the other three joined, two by capture, to make the Evenlode-Upper Thames. Then came the attack from outside—the headward growth of a Bristol Channel stream draining to the south-west.

IV. *The Growth of the Severn.*—Not to go too far back, it is probable that the Jurassic rocks which stretched from north-east Somerset to Glamorganshire had much to do in determining the position of the Severn. Possibly a western extension of the Carboniferous Limestone of the Mendip axis—represented by the Holmes—strengthening the Jurassic rocks, prevented or rather delayed headward growth of what may have been a small river with a strongly tidal estuary; and this might have caused the water to be turned south-eastwards to destroy the Jurassic rocks of Sedgemoor and Bridgewater.[3]

But the barrier was broken down, and the Taff and Rhymney were captured. Next the Usk system was obtained.

The largest and most important obsequent stream ought to be found on the south side of Dundry; but it is not—the Avon on the north side is so. That is to say, the longest obsequent has been developed in the valley of the later beheaded consequent, where there has been the

[1] This subsequent stream was developed along an anticline.

[2] The river-name in brackets indicates the predecessor which occupied the valley and flowed in the other direction.

[3] Probably the Dorset Stour represents the direction of the drainage just south of the Mendips, and it originally headed in Glamorganshire, crossing the non-existent Bristol Channel. Then the Parret, the Yeo, etc., are the obsequent streams developed when the Channel, or perhaps it was then only a south-west flowing stream, headed back towards Burnham. It will be seen that the obsequents have had time to become as long as the consequent Stour.

less time for the work. The probable explanation is that the date of capture was not very different, and that the Usk-[Avon] valley was originally the deeper, having received the larger drainage; and so it gave the obsequent stream a better chance.

The Bristol Avon has become the longest obsequent branch of the Severn north of the Mendips. In working back it takes off the waters of the Kennet, and prevents much westward growth of the west subsequents of the Salisbury Avon.

The long eastward course of several Avon tributaries, especially on the south side, points to former eastward flow of the Avon's predecessor. The tributaries join the present Avon in more or less opposition to its stream.

After the Monnow lost its head to the Middle Wye it is possible that it was itself captured by a subsequent branch developed northwards from its then stronger companion the Usk-[Avon]. And when the Usk group was captured by the Severn, this subsequent, which could give a much quicker fall, continued to grow northwards to Hereford, and captured the Wye group, thus beheading the Stroud valley stream. This subsequent would have been developed in Mesozoic rocks from about Chepstow, somewhat along the line of the present Lower Wye; but, probably, the difficulty of cutting through Carboniferous Limestone prevented the Severn working back in that direction, and it found it easier to break through the Palaeozoic barrier at Tortworth,[1] whereby it gained the Forest of Dean drainage.

The remarkable horse-shoe curves of the Wye can scarcely be accounted for by this history. They had perhaps been previously developed in an earlier river flowing over a low-lying area of Carboniferous Limestone during Jurassic times. The valley of this river was subsequently buried beneath accumulating Mesozoic deposits. After the capture of the later stream this infilling was easily re-excavated.

It was pointed out to me by the late Edward Wilson that the Clifton gorge of the Bristol Avon was first excavated in Triassic times, because it contains Dolomitic Conglomerate; therefore the lower part of the Bristol Avon occupies an old pre-Jurassic channel. A similar history may attach to the present Wye.

As the Severn captured the Forest of Dean drainage it started obsequent streams towards Dursley and Stroud. The Stroud obsequent —the Frome—has gradually abstracted the head-waters of the true Thames. Very noticeable is the elbow of capture, where the eastward working-back Frome has broken into small south-eastward flowing Thames consequents, and directed them to itself.

According to the theory the Frome should not be so important an obsequent as the Bristol Avon, because it has not had so much time to work back since its predecessor was captured,—and it agrees.

[1] That it thereby followed the strike of weak strata may have been an important factor.

When the Middle Wye was turned into the Severn system it still continued the northward subsequent, which of course may have been initiated as a tributary when the Middle Wye belonged to the Thames system. That subsequent captured some of the Lug, which, however, seems to have already suffered from the original Coln (Upper Teme). The state of affairs at this date is shown in the sketch map, Fig. 8.

Meanwhile the Severn was working its way north-eastward, and it successively captured the different portions of the Coln group. It must be supposed that before this took place the Teme had developed a

FIG. 8.—Sketch map. Courses of the streams just before the victory of the Severn in the Vale of Gloucester. Brought about—1, by adjustments of Thames streams among themselves; 2, by adjustments of the South Welsh streams among themselves; and 3, such adjustments in the latter case induced by the headward growth of the Severn. Scale about 20 miles to 1″.

northward subsequent towards Stourport, and beyond, so that when the Coln—into which the Teme then flowed—was captured, this subsequent rapidly developed, and obtained as much of the Upper Severn as had not suffered from the depredations of the Dee. If this had not been the case the Severn would have taken a more northerly or north-easterly direction.[1]

[1] North-eastward, somewhat in the direction of the Warwickshire Avon, should have been the main and largest stream of the Severn, working along the strike of weak rocks. Now it has what may be called a somewhat lopsided appearance—an abrupt change of direction at Tewkesbury. So, it will be noted, it had at a prior stage (Fig. 8). That the Avon did not develop faster is due to the proximity of sea on east England favouring successful growth of the Trent, etc., otherwise the Avon had worked back right into Yorkshire.

Having captured the Coln, the Severn started an important obsequent stream up the valley.

Comparing the obsequent Chelt with the obsequent Frome, it will be seen that the former has done less than the latter, which agrees with the date of capture. The Frome has worked back and captured various consequent streams on the Cotteswold upland; but the Chelt has not yet done anything in this way. It is in a less advanced stage of obsequent development than the Frome. It should have captured the present head-waters of the Coln, but it has not yet done so. It will do so in time, but by then perhaps the Frome will have got the Churn at Cirencester.

I had the advantage of visiting the Upper Coln with Professor Davis. He pointed out to me some most instructive phases in its valley development, corresponding with decrease in its volume. There are three valleys. In the upper valley there are meanders of large

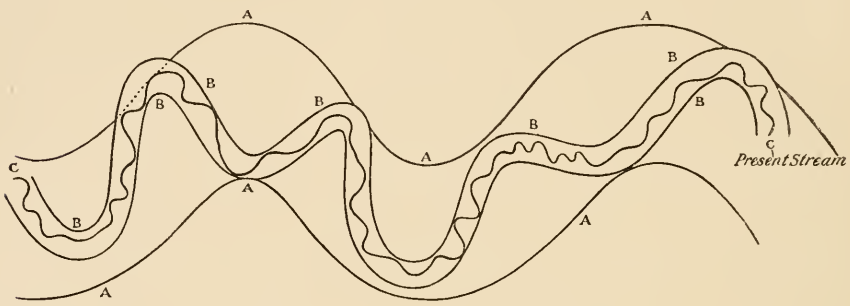


FIG. 9.—Diagram of the phenomena in the Coln valley south of Andoversford. The curves are only very approximate. A A A, curves of upper valley; B B B, curves of lower valley; C stream wriggling in its present valley.

curve, such as a river of large volume would make. In the lower valley the diminished river has been unable to follow the large curves, and it made two turns in each original meander. In the lowest or present valley the river is taking a wriggling course inside the smaller curves. I append a diagram (Fig. 9) to show what has happened; but it requires a map with contour lines at every 25 feet to illustrate clearly such details of river-valley development.

Diminished river-volume is due to two causes—diminished rainfall and diminished drainage area. Allowing that the former has taken place, yet it will perhaps hardly account for all the phenomena.[1] Diminished drainage area is what coincides with the theory set forth. Thus the big curves were cut by the large river, which with its west branch drained the country west of Cheltenham and Gloucester, and by its northern branch—in the Charlton Abbots valley—drained the country north of Winchcomb. The smaller curves were cut by the

[1] The so-called pluvial period of post-glacial times will not fit here, except to the details of the lowest valley; for the date of the upper valley must be Pliocene.

river when it had lost all its western branch, and only received the drainage down its northern valley. In turn its northern branch was beheaded, and its drainage-area further and further diminished by the growth of the obsequent Isborne. So the river is reduced to its present small volume—a little wriggling brook.

Having stood on the Chiltern Hills overlooking the Goring gorge and noted the details of that scene, it is not difficult, when subsequently viewing the Cotteswold escarpment and the old West Coln breach from the south end of the Cleeve Hill plateau, to transfer the details of the former scene to the latter, and imagine the aspect of the Cotteswold uplands and river-system when its stage of development was the same as now belongs to the Chalk escarpment. That is the picture of what the original Coln river-system—the river which cut the big meanders—must have looked like before its capture by the Severn. Between Cleeve and Leckhampton Hills there was a broad river-valley perhaps some 250 feet deep. In that valley a river quite as big as the present Thames at Goring; and stretching away to the west, right to the Malverns, there was a broad plain of Lias about 250 feet below the Cotteswold escarpment. The Upper Lias corresponded to the Greensand series, and the Lower Lias to the Oxford Clay of the Oxford district. And across this plain ran the original Coln, much in the same way as the Thames now crosses the Oxford Clay before entering the Chalk Hills; also in this plain there had been developed subsequent streams corresponding in all respects with the Ock and Thame. The stage of river-development now to be seen in the neighbourhood of the Goring gorge was once the stage of river-development appertaining to the gorge which now lies west of Cheltenham. Here this stage of development passed into a new phase by the successful working back of the river Severn.

After what may be called this Severn victory in the Vale of Gloucester, the further development of the river-system may be shortly told. It started or strengthened two subsequent branches—one northwards which captured the Shropshire and Welsh drainage, the other northeastward—the present Warwick Avon[1] which cut off the head-waters of the Thames tributaries from the north; and has developed obsequent branches like the Isborne and the Stour to further shorten the Thames system. While the Thames was thus suffering from the successful growth of the Severn, it had also experienced considerable loss from other rivers. In the north-west its head-waters had been taken by the Dee, Weaver, Mersey, etc. In the north it had suffered from the Trent, as well as from the three parallel subsequents, the Welland, Nen, and Bedford Ouse. From the latter it had suffered very considerably, especially in regard to tributaries which enter near London.

[1] From the lie of the ground south of Tewkesbury it seems reasonable to conclude that the Avon formerly joined the Severn a few miles south of Tewkesbury, and that the present junction at Tewkesbury is a comparatively recent development.

As time goes on the Thames system will suffer most in the west, by the growth of Severn obsequents like the Bristol Avon, Frome, and Chelt, which can give the Cotteswold drainage quicker fall. It may suffer in the south by the growth of the Salisbury Avon.

The Severn system will suffer in the west by the growth of west Welsh streams; and the Severn itself may suffer by the shifting of its drainage westward—thus probably the Usk, further developing subsequent streams working in the Old Red Sandstone, will capture the Monnow and the Wye, giving the latter an easier channel than through the gorge of Carboniferous Limestone.

*Conclusion.*—To prevent misconception, it may perhaps be desirable to say that "a stream" may be of water or of ice. The latter will follow the drainage lines marked out by the former; it will not make unaccountable changes of direction. Its chief feature is perhaps to overflow its valley; and this might greatly aid the headward growth of an attacking rival.

Many of the statements in this paper may be termed mere speculation, with very few arguments. It is fully admitted. To pursue the matter further, I want much better maps than are now to my hand. I require—(1) A clear river-map; (2) a similar river-map with contour lines, and, in figures, the height of all hills and passes; (3) a similar river-map showing the solid geology, and particularly the dip of the strata; (4) a similar river-map showing the drift geology. There is of course much to be done in a study of the drift in this connection. That has been shown by J. Prestwich (8), J. W. Gregory (9), and Osborne White (3), not to mention other authorities. Therefrom can be obtained valuable aid in regard to the chronology of river-valleys and the direction of old streams.

Thus in the Moreton valley, just on the present divide—at the head-waters of the Evenlode—there is a mass of drift. It shows the Evenlode to have been a big river, that it drained Triassic country, that its waters froze in the north. The ice floated down, bringing Triassic débris, and it melted in passing along the Moreton valley, dropping the débris endwise into a muddy river-bottom. Much of this drift perhaps extended back over Warwickshire, up the original river; and when the Avon broke into this country, taking off so much of the original river as the depredations of other streams had left—which was little enough—it perhaps attacked this gravel, and has been engaged in transporting it westwards, taking it to Evesham, and perhaps into the Severn valley. And the Stour may have been engaged in retransporting northwards what the original Evenlode had taken the trouble to bring southwards.[1]

The Lower Severn has cut down its valley very much since what

[1] This means that the excavation of the Avon valley, below about a 500 feet contour line, has been accomplished since the deposition of the Moreton drift. What then is the date of that drift? (later Pliocene, when the climate was getting cold).

is called "Northern Drift" was deposited. The Upper (Shropshire) Severn has done very much more. But perhaps there are two deposits confused in the term "Northern Drift"—a high-level drift, and a retransported low-level drift.

The question why Triassic débris is found on the Moreton divide, and not on the Chelt or any of the western divides, may be easily answered. Rivers flowing from the west of May Hill, Malvern, etc., country would come—at the level of the Chelt divide—off Palaeozoic rocks straight on to Lias.

But other queries in this connection will occur, to which answers will be necessary.

The date of an obsequent river like the Frome in the Stroud valley can be approximately fixed by the gravels. In the valley just below Stroud there are gravels with mammoth remains. And so the obsequent valley had been deeply, and it would seem widely, excavated before that date of Pleistocene geology. Since that date the river has lowered its bed some 40 or 50 feet.

In the Chelt valley, near my house, are several gravel pits. Up the valley there is almost entirely Jurassic, mostly Oolitic material. Coming down the valley there is found a greater and greater admixture of a quartz sand. Beyond the valley there is wholly sand. Farther west there are Triassic materials.

The sand mixed with the Jurassic gravel is not local—not from the Cotteswold Hills; it is probably from the Trias rocks. How it was brought up the Chelt valley may admit of more than one explanation.

There is much to be done with the gravels. After the Severn had, as it is supposed, captured most of the western drainage, and had started the Avon into the Midlands to see what it could filch, there then remains a long period of history attached to the widening and deepening of the Severn valley.

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# The True Interpretation of Lamarck's Theories: A Plea for their Reconsideration.

By R. F. Licorish, M.D.

That Lamarck has been misinterpreted and misunderstood from his day to this, appears to me to be due to two causes chiefly, first, the lack of true biological and physiological knowledge in his day, which led not only to the rejection of his theories by his contemporaries, but also gave origin to the second cause of misinterpretation, viz. the obscurity and ambiguity of his language.

Having succeeded, as I believe, in developing his true meaning, I now desire to bring my views to the notice of evolutionists, believing that this interpretation will make clear many obscure organic phenomena, as well as enable us to do full justice to that great philosopher.

In summing up the opinions of biologists on Lamarck Mr. G. Sandeman thus writes ("Problems of Biology," p. 153): "It is certain that this biologist has been, on the whole, misinterpreted, but, on the other hand, it is not easy to be sure that one understands him. And the confusion has taken place chiefly over that key-word of his system —*besoin*, or need." Again he states: "It—*besoin*—probably appeared to Lamarck to have a quite definite meaning requiring no further analysis."

Mr. Sandeman is quite right in the above statement, for, as I shall show, the whole misinterpretation of Lamarck is due to the non-perception by biologists of the true meaning of this need or desire. Moreover, it will be pointed out that Lamarck himself failed to see the true interpretation in some respects, and hence the full significance of his important conclusions and laws.

Mr. Clodd has well said ("Story of Creation," p. 93): "The functions of living things are threefold—nutrition, reproduction, and relation, in other words to feed, to multiply, to respond to the outer world." There is, however, one special relation which, although included in the last of those, yet requires more attention than Mr. Clodd has bestowed on it, viz. the relations which living things have with other living things, especially as regards protection.

At this stage, it is well to consider carefully the nature of functions, because we cannot understand Lamarck unless we keep in

view the functional life of living things, and endeavour to understand clearly the true nature of a function. A function is the use which an organ serves. All living things possess organs, the uses of which are called functions. Moreover, there are functions which require, not only their proper organs, but also other auxiliary organs for their effective performance. But functions, and hence organs, vary in importance. There are certain functions so highly important to life and the propagation of the species, that special centres in the central nervous system are found to preside over and control them, giving origin to the need or desire for satisfying them. Such functions are—nutrition, reproduction, and, to some exteut, protection, and it is in response to the need or desire of the organism that such functions are satisfied.

It is doubtful if any one will be found to dispute the importance of those functions, or their connection with the need or desire emanating from the brain for their satisfaction.

Now it is held as a truism by biologists that "the function makes the organ." That this is so must withstand any attempt at refutation, for it is inconceivable that the organ was first to appear, to be subsequently followed by the function. Stress is laid on this point, because the whole interpretation of Lamarck hangs on its recognition. Further, we can well understand that the nature of the function, *i.e.* the manner in which the need or desire presiding over the function is satisfied, determines the structural nature of the organ. Change the method of satisfying the function and we may change the structural nature of the organ. But we must also remember that the most important functions have not only their proper organs, but also other auxiliary organs which are brought into new uses when the methods of satisfying the functions are changed. If, *e.g.*, an animal subsisting on surface vegetation, etc., be driven to climb trees in order to satisfy its desire for food, as it is said the mongoose in Jamaica now sometimes does, while the true organs of nutrition will doubtless undergo little if any change, yet, if the habit through any cause, such as continuous drought, etc., be fully established, the auxiliary organs—the feet and claws—will gradually be modified until they serve fully the function of nutrition, *i.e.* until the feet and claws fully serve the purpose of procuring food. We should then get undoubtedly, in the case of the mongoose, a change towards a squirrel-like type. We thus learn how correlated are different and distant organs in maintaining the integrity of the most important functions.

Bearing the above facts in mind, we are now prepared to consider the theories of Lamarck. These theories he condensed into certain formulæ and laws, which are as follows:—

(1) "That every change which is at all considerable and continuously maintained in the circumstances of each race of animals, effects in it a real change in their needs."

(2) "That every change in the needs of animals necessitates other actions on their part for the satisfaction of the new needs, and, in consequence, other habits."

(3) "That since every new need requires new actions to satisfy it, it demands of the animal which experiences it either the more frequent use of such a part as was formerly less used, so that it becomes considerably developed and enlarged; or the use of new parts which insensibly arise in the organism from the needs, *by the efforts of its inner feelings*, as I shall presently show from known facts. And to arrive at the true cause of so many different forms, and so many various habits, as are seen in the animal world, one must recognise that the infinitely diversified but slowly changing circumstances in which the animals in each race have successively been placed, have brought about in each race new needs and consequently changes in their habits. As soon as one has recognised this incontestable truth it will be easy to perceive how the new needs can have been satisfied, and the new habits taken on, if one attends to these two laws of nature, which have always been corroborated by observation."

"*First Law.*—In every animal which has not passed the limits of its development, the more frequent and sustained use of any organ gradually strengthens that organ, develops it, increases its size, and gives it a strength proportional to the use in question; while the constant disuse of such an organ insensibly weakens and deteriorates it, progressively diminishes its faculties, and finally results in its disappearance."

"*Second Law.*—All that nature has caused to be acquired by, or lost to, individuals through the influence of the circumstances to which their race has long been exposed, and therefore through the predominant use of an organ, or through the constant disuse of a part, she preserves by reproduction for the new individuals which come from them; provided that the acquired changes are common to the two sexes, or to those which have produced the individuals."

Before entering on the further explanation of Lamarck's key-word, *need or desire*, I have to point out an almost fatal error in Lamarck's language when stating the above conclusions. Lamarck therein speaks of *new needs*, but careful consideration of his conclusions has enabled me to see that he really did not mean *new needs*, but that he referred under such a term to new methods of satisfying the customary needs of animals, as *e.g.* the need of food, etc. Instead, then, of speaking of *new needs* he should have said *new uses* of parts in order to satisfy the functional needs of the animal. This error has been, I believe, a stumbling-block to many in interpreting Lamarck. I myself did not see the error until long after I understood the true meaning of his need or desire. Corrected in the light of this discovery his conclusions read thus:—

(1) That every change which is at all considerable, and continu-

ously maintained in the circumstances of each race of animals, may effect in it a real change in the methods of satisfying their needs, and hence new uses of parts hitherto used in a different way.

(2) That every such change in the methods of satisfying their needs necessitates other actions on their part, and in consequence new habits involving new uses of parts.

(3) That since every new method of satisfying their needs requires new actions, it demands of the animal which experiences it either the more frequent use of such a part as was formerly less used, so that it becomes considerably developed and enlarged, or the use of new parts, which gradually arise in response to the desire or need. And so to arrive at the true cause of so many different forms, and so many various habits, as are given in the animal world, one must recognise that the infinitely diversified and slowly changing circumstances in which the animals of each race have successively been placed have brought about, in each race, new methods of satisfying their functions, and consequently new habits, including the new uses of all correlated parts affected thereby, etc.

As regards the two laws of Lamarck, they are correctly stated, and their truth can be perceived by paying attention to the above corrections.

A careful study of the above conclusions as corrected, and comparison with Lamarck's own statement, must put their signification in a somewhat different light from that suggested by his language.

From what has been already stated, it must be understood that what Lamarck meant by *need or desire* was not, as is usually understood, an *indefinite* need or desire, but those needs or desires of animals which lead to the satisfying of their most important functions to ensure their existence and propagation. I myself did not at first understand Lamarck, but on reading of the reversion of the tame pig to the wild boar in the New World, the true meaning suddenly flashed on me in this way: One of the most significant changes in the wild boar from the tame pig is as regards the ear, which in the former is small and erect, and in the latter large and pendent. On reading this statement, my mind instantly saw the analogy between the ear so modified in freedom, and the semi-erect ears of half-tame pigs in the West Indies. I had frequently seen on such pigs the effect of a noise: they would at once erect their ears and scamper off; and I soon saw that the tame pig, escaping from the Spanish settlements in America, would, in order to avoid danger, have more and more frequently to erect their ears to catch the earliest signs of danger, and that this frequent erection, in order to protect themselves, would gradually cause the ears to lose the superfluous fat and develop muscle, with a corresponding decrease in size, until the ear became like that of the wild boar, and thus efficient for the purpose it served in the catching of the earliest sign or sound of danger; and as the modification of character was an innate one—that

is, in response to the need or desire of the animal—the ear, through heredity, would gradually revert to that of the wild boar. And in a similar way the tame pig would gradually lose all its character due to domesticity, and assume those compatible with a wild life. And we see the same gradual modification of ear in dogs that hunt by scent (*e.g.* the blood-hound), the ears, relatively little used, gradually enlarge, and become more and more pendent.

It may be thought that the foregoing has reference only to "use and disuse," and the modification of characters induced thereby; but to take such a limited view of the changes brought about, in accordance with Lamarck's laws, would be wrong. The laws of Lamarck have reference to changes of a far wider and deeper nature than is understood by the use of the phrase "use and disuse." The modifications of characters produced by changes in the circumstances of animals bear, not only on the use and disuse of special parts, but also on all other auxiliary organs which co-operate in satisfying the wants or needs of animals, which changes in their circumstances have affected. When we consider the many different parts or characters which are brought into co-operative action when an animal, through a change of circumstances, is forced, through its desire for food, to adopt a new method of feeding—when we consider, also, the changes in the nutritive organs, which a radical change in the nature of the food may entail—we begin to realise the many changes, external and internal, which changes in the circumstances of animals may bring about.

Lamarck's theory, as here developed, explains how, granted that a change of habits in any species may produce changes in special as well as correlated organs, and may affect, not individuals only, but a species as a whole, new species appear in a comparatively short time, owing to the fact that all such modifications of characters are fixed through heredity, thus accounting for the absence of transition forms in many instances—a fact which research proves.

It may be well to state how, by the Lamarckian theory, the two kinds of characters, innate and acquired, are explained. Innate characters, on this theory, are those derived through the wants or needs of the animal, *i.e.* through the central nervous system. As they are brought about *en masse* they affect both sexes alike; hence, through heredity, they become fixed, and the species is modified thereby. Acquired characters, on the other hand, are derived through the surrounding conditions, such as climate, etc. Their relation with the nervous system is only through reflex nerves. When the whole of the reflex nerves are affected, as in the case of a tropical sun on the skin, in process of time the central nervous system becomes modified, and the acquired character becomes fixed or innate. On the contrary, characters such as mutilations and scars, which only affect a few of the reflex nerves, do not become innate, or capable of being transmitted, because the unaffected reflex nerves, being in the vast majority, maintain the integrity of the

reflex nerve centre—of even that portion pertaining to the affected nerves.

The gradual change taking place in the ear of the tame pig, in response to its desire to escape its enemies in its reversion to the wild boar, is a good example of an innate character; the gradual tanning of the skin, or darkening of the complexion of the Boers of South Africa, a good example of the conversion of an acquired character, affecting the whole of the same reflex nerves, into an innate one; the non-transmission of scars, etc., a good illustration of an acquired character, which, only affecting a few reflex nerves, does not become in time innate, and so capable of being transmitted, except, perhaps, in very rare cases. Innate characters are voluntarily derived; acquired characters, involuntarily.

Barbados, West Indies.

# The Case of Doctor Otto Kuntze.[1]

"Y a-t-il une charlatanerie plus grande que de mettre les mots à la place des choses, et de vouloir que les autres croient ce que vous ne croyez pas vous-même?"—VOLTAIRE.

ONE recalls the noisy campaign, undertaken by Doctor O. Kuntze in 1891, and continued in the following years with a view to regenerate botanical nomenclature. By means of a plausible interpretation of the Paris Code, and by grossly altering several of its fundamental articles, M. Kuntze succeeded in giving to his schism a varnish of legality. In spite of his eccentricities one discussed him with more or less deference. Slashing and pugnacious, blowing his own trumpet, obstinate in his fallacies, he rode, not without bluster, his hobby of *priority at any cost*. Putting words in the place of facts, as Voltaire says, Kuntze substituted for *definitions*, prescribed by the Laws of 1867, his trick of *identification* (*Recognoscirung*), and thus resuscitated an infinity of generic names, still-born according to Article 46 of the Code, applying to them his new scale of *demi-nudity* and that of the *fractional priority of* 50 *per cent*, absurdities combated by Alphonse de Candolle up to his last breath.

This resulted in letting loose a mass of about 30,000 species, and about the same number of *nobis*. These *nobis*, according to their author, mattered little to him, so little that several times over he has declared he could only condescend to make concessions on some points of secondary importance, on the condition of acceptance in their entirety of all his "reforms," that is to say all his *nobis*. Besides, his real aim was more elevated. To establish an immutable nomenclature *ad æternum*, to cut short all disputes on detail, to render every author his due (especially those who have left simple labels)—such a result could only be obtained by iron rules, much more particularised than those of 1867, and provident of endless litigious cases.

Unhappily for Dr. Kuntze, his neo-code did not appear, to the bulk of botanists, based upon "des motifs assez clairs et assez forts pour que chacun pût les comprendre et les accepter" (§ 2, *Laws*). Many of the premisses seemed arbitrary, many of the consequences doubtful, and it was considered annoying that Kuntze wished to impose them on the world, contrary to the 2nd and fundamental article of the Laws

[1] By E. Levier. 8vo, *Florence*, 1898 (Nov.): especially translated for this journal.

voted upon at Paris. It was considered that the frightful disorder brought about by the neo-code would be a hundred times worse than the evil which it pretended to remedy. In fact, from the uncertainty on the subject of a relatively limited number of names, diminishing year by year, thanks to the work of systematists, one falls into complete disorder, into endless ambiguities, into quarrels over useless names, and into envenomed polemics, all which things Article 3 of the *Principes dirigeants* of the Laws counsel one to avoid as one avoids the plague. It was noted, with satisfaction, that Alphonse de Candolle, whose competence in nomenclature is at least worth that of Otto Kuntze, disapproved of many of the theses of the innovator and declared them inacceptable "avec des raisons claires et assez fortes pour que chacun pût les comprendre et les accepter." It was a case of much ado about nothing. The great clamour raised around the *Revisio generum plantarum* gradually subsided; systematists recovering from their first stupor, once more resumed their labours, gleaning here and there in the Kuntzean repertory a few names recognised as valid, and passing the rest over to the archives.

Otto Kuntze did not found a school. No marked and competent botanist, not a group of monographers or systematists, either in the Old World or in the New, adopted fully, freely, or without reserve his rules and his innovations. The Rochester Code, which is the sole direct and collective emanation of Kuntzeism, adopted only the most unfortunate of his superfoetations on the Laws of 1867, *nomina seminuda*, rejected everywhere else, and equally rejected by zoologists. The scientific men who subscribed to the nine articles of the Rochester Code (and they are nearly all the botanists of North America) rallied, by way of revenge, round the starting-point of 1753, scouted by Kuntze under the sobriquet of "*initium ignorantium*," and on several other points set themselves in open opposition to their ex-master, who spared them neither scratches nor epigrams. We learn to-day, from Kuntze himself, that Article IV. of the Rochester Code (*Homonyms*) and the principle of "*Priority in place*" will compel their authors to change the names of 23,300 species. Kuntze, for his part, needing not only more than 30,000, but more than *40,500* changes, in great part different from those of the Rochesterians, the language of flowers will offer this pretty spectacle: the names of the Americans will be unintelligible to Otto Kuntze, and *vice versa*, whilst those botanists who remain faithful to the Paris Code will have to search out the meaning of about 60,000 schismatic and hieroglyphical species. Hash, chaos, Babeldom—that, in seven years, must evidently be the result of the shock given to systematic principles by the *Revisio generum plantarum*. The indifference into which a pugnacious book falls being the worst of deaths, Kuntze judged the moment had arrived to beat his tom-tom again. An advertising pamphlet, with flattering notices on the last page, attesting the excellence of his wares, was distributed *urbi et orbi*,

to announce the publication of a fourth volume of the *Revisio*. With an alluring title-page for readers eager for scandal, this puff is seasoned with some of the tastiest specimens of the last nomenclatural creation of Kuntze, aiming no longer at plants, but at botanists. The new nomenclature is codified, very simply as we shall see. There are only two Articles :—

§ 1. All those who accept the botanical names of Doctor O. K., or who accept 70 per cent of them, are honest men.

§ 2. All those who are against the botanical names of Doctor O. K. are "ignoramuses," "pettifoggers," "confusionists," "liars" (Lügner), "infamous forgers," "downright idiots" ("senile" and soft-headed), "Jesuits," "sheepsheads," that is to say "imbeciles," "idiots"; better than that, "jugglers," "harlequins," "swindlers," "pickpockets," "dangerous quacks," "bullies," and above all "anarchists."

All these qualifications between commas are culled *passim* from the four volumes of the *Revisio*. There are some more spicy still, but I pass on.

What a relief to the camp of the "executed," and what a riddance! A man of science who gives his reasons and defends them, even with vivacity, one takes seriously. One admits, occasionally, a stronger word, provided it does not smack of the gutter. One inoculates oneself, if need be, with the anti-rabic views of Pasteur, before fighting a demoniac hydrophobe. But how can a man protect himself against the non-sentient eruptions of a mud-volcano? Let the mud fall down again naturally on the mouth of the crater! This is the way of mockers, and, in such cases, of wise men. As regards naturalists, curious by temperament and charitable by duty, they examine the pathological side of a case, and discover therein reasons for indulgence.

For some time, the dictatorial manner of the volcanic doctor, his magniloquence, his vicious habit of conducting himself like a "grande puissance traîtant de pair à pair avec l'universalité des autres botanistes" (Spitzer), his explosive susceptibility, have appeared alarming signs. The suspicion that he had a bee in his bonnet was supported by another symptom, nearly always the concomitant of ideas of grandeur: the fixed idea of persecution. In fact, in several notes in his book, of which one is entitled *Falsum infamans Aschersonii*, Kuntze relates how Paul Ascherson has been persecuting him (*verfolgt*) from time immemorial. Here are the facts (*Rev. g. pl.* iii. note 84): At a flower show, got up by twenty botanists of Brandenburg, a competition was held as to who could find the most novelties. But the victor's palm, instead of falling to Kuntze, was "insidiously" disputed by Ascherson. This little story goes back to 1865. Kuntze adds that he was by no means ambitious for the victor's palm. It is on

that account, perhaps, that he has never stomached this first "persecution." We note that the "*falsum infamans Aschersonii*" treats of a very minute question of —— consonants. M. Ascherson had hazarded the opinion that, according to the new Kuntzean rules of orthographic licence, certain assonances, as *alpinum* and *albinum*, were liable to cause the rejection of one or other of the two names. Thereupon, blue fury of Kuntze. It was, according to him, more than an insult, it was a falsification covering him with infamy.

One sees the morbid outrageous disproportion between action and reaction. Reflex actions in Kuntze are enormous. A mere nothing upsets them, just as it is quite enough to tickle the epidermis of a strychnined frog to put it in a state of opisthotonos. Dr. Kuntze is normally, perennially strychnined. His inhibitor centres have forgotten to allay the effects of ascending excitation; as I say, he has lost his moderator nerves. *Albinum* for *alpinum* makes him writhe like a mountebank; his anger cannot contain itself, and he has no respect for the hardly closed tomb of M. Alphonse de Candolle. Let us quote him (without correcting his misprints):—

"Les notes suivantes etaient finies avant la mort d'Alph. de Candolle; jè préfère de les publier sans aucune changement. (O. Kuntze, *Rev.* iii. note 20.) C'est une arrogance ridicule! On a déjà accepté en part (*sic*) de mes noms nouveaux dans plusieurs livres botanique des deux hémisphéres. Que voulez donc M. D. C.? (*sic*). La plus grande quantité des noms que vous croyez erronément synonymes sont le résultat des 'Lois.' *Et moi je connais ces lois*, comme vous verrez dans mes autres notes, *mieux que vous-même* [c'est nous qui soulignons]. Vous les avez oubliées et vous faites des lois nouvelles fantastiques que vous croyez loyales (*sic*) parce qu'elles viennent de vous. Mais après la modification et l'acception (*sic*) de vos propositions par le Congrès, elles ne sont pas plus les vôtres (*sic*); elles sont devenu les Lois et vous avez à leur obéir aussi bien que tout autre citoyen."

The police at once!

The inverse case is found in impulsives when their *amour propre* is agreeably tickled. Kuntze puts first, in his puffs, a statement of M. G. Poirault, in which he has detected some commendation. Let us judge of the commendation:—

"Ainsi, pour différentes raisons, et par des procédès divers, M. O. Kuntze est arrivé à changer 1074 noms de genre et de ce fait c'est environ *trente mille* plantes qui reçoivent des noms nouveaux. *C'est beaucoup mais il paraît que c'est au plus juste* [c'est Kuntze qui souligne!!]" et plus haut, "*Urbanisol* O. K. (remplaçant *Tithonia* Desf.) a je ne sais quel reflet d'Orient . . . et, si des noms comme *Schweinfurthafra* O. K., *Müllerama* O. K., *Hasskarlinda* O. K., avaient chance d'être adoptés, on verrait de suite que le premier de ces genres est africain, le second américain, le troisième spécial à la péninsule indienne. M. Kuntze excelle à arranger les choses le plus simplement et le plus

brièvement possible.—Il est à souhaiter *qu'on oublie le novateur* [c'est nous qui soulignons] pour donner du monographe toute la considération à laquelle il a droit." (*Journ. de Botanique*, Mars, 1892.)

This translucent panegyric has been swallowed whole by the candid doctor, who, clumsily and lamentably, has fallen into the trap set for his immodesty. After that, it is quite fitting for him to call others "imbeciles," "idiots," and "sheepsheads." Need we have another example of the cock-and-bull stories, and the laughable illogicisms into which impulsives of Kuntze's kidney fall without suspecting it?

"M. Levier," writes this hot-headed doctor (*Rev. g. pl.* iii. p. 51), "me provoque dans sa *Rectification* (*Bull. Herb. Boiss.* 1896, p. 575) au duel littéraire, mais on ne se bat pas contre des idiots, on les traite tellement qu'ils ne puissent plus faire de mal, ce qu'il est de mon devoir de fair maintenant." (Listen to the method) "Il m'a donc autorisé à l'appeler Schafskopf, conformément il a donné plus tard ses jugements avec une infaillibilité soubite (*sic*) sur beaucoup de question de la nomenclature sans en avoir la capacité, tout en usant de sa verbosité inquiétante et de sa fantaisie; mais, en considération de l'arrogance importune de ce Schafskopf et en considération de son artifice et de la fausseté extraordinaire qu'il a prouvées plusieurs fois, il ne serait point injuste de le stigmatiser comme *charlatan dangereux*."

After this thrust, one would perhaps think me dead, and my evil-doings cut short. Not the least in the world. In his picturesque gibberish Otto Kuntze goes on:—

"Il (Levier) est aussi dangereux, parce qu'il va empêcher un Congrès compétent pour s'épargner la correction de sa régle à part (*sic*) et l'application de ses idées fixes."

Very good. I can rise up, with all the holes drilled me, in the omnipotence of a demi-god, having all the botanists of the world in my pocket! Kuntze, decidedly, has no chance with his "Sheepshead." His sharpest arrows return cruelly upon himself, and he *sheepsheads* himself vigorously. But a truce to gaiety; let us neglect even this neologism: auto-sheepsheadification, worthy to be by the side of *Biscogneauxia* O. K., of *Müllerama* O. K., of *Peckifungus* O. K., etc., and—courtesy for courtesy—let us compliment Jupiter O. K., on his double-escapement thunderbolts, good for killing, and better for restoring to life.

The energy of Doctor Kuntze is inexhaustible; the manifold talents that he displays to make laughter at his own expense, appear as a diamond of a thousand facets which each throws its own bright flash. Never short of burlesque and acrobatic inventions, after having committed *Kochneago*, *Lippomuellera* (blear-eyed Mueller), *Cesiusia*, *Pasaccardoa*, *Radlkoferotoma*, and other quips, the imaginative doctor has searched further and found better. He has surpassed himself. The plum of his pie (after *Tripocorynelia tripos* O. K.) will without contradiction be the "*Trifolium charlatanieum*, avec les trois variétés *levierianum*, *lejolisianum*, *aschersonianum*—les types!—de plus nov.

var. ? *malinvaudianum*, véritables herbes mauvaises qui pulluleront à Paris en 1900."—"Les malheureux! Voilà ce qu'ils font du langage des fleurs!" observes, *à propos* of this trump card, the *Journal des Débats*, 14th October. *Et nunc erudimini.*

Have we been sufficiently absurd, O my learned friends, to take literally, for six years past, the rhodomontade of this old bogie, eater of botanists. His savage roarings, his round matador eyes, the dreadful twirls of his cabbage-cutter, hid a humorous and sportive nature. Like the street mountebanks who pretend to swallow snakes and swords. Scratch the sword-swallower and you will see revealed the chalked nose of the clown so dear to infancy.

But our Old Bogie is not comfortable. Every time he conjures up the menacing spectre of the 1900 Congress, he beholds the repropagation of the tare that his claws have just destroyed and he loses the thread of his ideas. An irritating vision haunts him—that of an assembly where one will demand better arguments than "pickpocket," "bully," "Jesuit," and "sheepshead," and where turbulent humbugs, demoniacs, retailers of pamphlets at twenty-eight marks the volume, will be summoned back to good manners. The "Trifolium" proves satisfactorily that Doctor Kuntze has still lucid intervals.

Not for the sake of Kuntze, but for those who may have succeeded in construing his gibberish into current language, I have brought to light one, or rather two calumnious assertions upon which this pure flower of learning returns with a fury and with hiccups of big words (fifteen pages of abuse) that denote a bad conscience.

He does not cease to write and to print that I have abominably violated a "contract" between us on the subject of our private correspondence. Now there is a unilateral contract and a bilateral one. To request some one whom you suppose to be a gentleman to hold as confidential a correspondence that you commence, is not a contract; it is a courteous appeal to the elementary rules of honour. M. Kuntze, on the other hand, considers a "contract" the following procedure: to accept the correspondence, to carry it on to the nineteenth letter, and then to break it off abruptly, with this ultimatum: "*Je vous défends de publier quoi que ce soit en fait de nomenclature, si vous ne faites pas paraître en même temps et in extenso toutes mes lettres* **et toutes les vôtres**" (this last part of the paragraph is adroitly suppressed in the *Revisio*, iii. (2), p. 58, where there is only a question of "reciprocity"). M. Kuntze having a lively interest in forcing my silence—one sees why—and in stifling my objections, found it quite natural to shield himself behind the privacy of my letters, and threatened me, in no equivocal terms, to divulge them in case of disobedience. This trap he decorates with the pompous title of "contract." Readers of the *Bulletin de l'Herbier Boissier* (1896, p. 575) know in what very categorical terms I have rejected this fool's contract, terms which have not prevented Kuntze from republishing the lying

assertion that I accepted it. But Kuntze has raised to an academical art the art of making authors say things that they did not say. I congratulate myself to-day that I did not shrink from the publication of some phrases torn from my early letters. They prove that M. Kuntze deludes himself and wishes to delude others in attributing to me hostile intentions from the beginning of our epistolatory relations. When one attacks in treachery and in secrecy (*hinterlistig aushorchen*), one weighs one's words somewhat differently. To have reduced an adversary short of arguments to this poor military device, to have put him in the shameful necessity of raking up confidential letters, keen sometimes but never uncivil, to respond to public criticism, to see him struggle like a madman in his virtuous rôle of slaughtered lamb, all for words, for halting names (noms à béquilles), for priorities more or less shadowy,—such a result is to encourage a third year's candidate in nomenclature. I do not grudge Otto Kuntze a real obligation for having guided my first steps. One is not born a nomenclaturist, as one is born a cook-shop keeper. The eloquence of his eighteen letters made me a model disciple, and his comminatory ultimatum has given me the heroic courage (Todesverachtung), which I should never have otherwise found, to bring before the public our interesting little discussions. Tu l'as voula, Georges Dandin!

The second folly of M. Kuntze consists in unwearyingly and everlastingly repeating that I have suppressed in my French translation of the Berlin report, made in great haste at Bormio in the presence of Professor Ascherson, who took it away to Genoa, the judgment of Viennese botanists, *favourable to the reform of Kuntze* and *unfavourable to resolution* vi. *of Berlin.* (81 Generic names debarred; among them: *Mokof, Brami, Ourcti, Gansbium, Hondbessen, Choeho, Belutta-Kaka* Ad.)

M. Kuntze pretends to be ignorant of, or believes the public sufficiently badly informed to be ignorant of, the following passage from *Atti del congresso botanico internazionale di Genova,* 1892, p. 104, line 6, below: "Un groupe de botanistes autrichiens, réunis sous les auspices du professeur v. Kerner *condamne notre projet* (iv.) au nom de la loi inconditionnelle de priorité, et tel paraît également le point de vue de M. Müller, Arg." (Rapport de M. Ascherson.) Is this clear enough? Where is the suppression of the *unfavourable* verdict? Does M. Kuntze now desire me to translate for the French public, passages from the Viennese document that he thinks *favourable* to himself? Here is the beginning:

"Les soussignés ayant pris connaissance d'une lettre de M. Ascherson à M. v. Kerner déclarent saluer avec plaisir l'initiative d'une démarche tendant, d'une part, à enrayer le *confusion causée dans la nomenclature botanique par l'ouvrage récent de M. O. Kuntze, Revisio generum plantarum,* d'autre part, à compléter les lois de la nomenclature. Les soussignés se saillirent sans réserve (*vollinhaltlich*) aux résolutions

ci-après (I, II, III)[1] et s'engagent, dans les questions de nomenclature qu'ils auront à traiter, à mettre en pratique les principes qui y sont formulés."

These three resolutions annihilate almost all the innovations and all the *nobis* of Kuntze, since they repudiate his starting-point of 1735, his *nomina semi-nuda* and the greater part of his rules for orthographic licence. All the end of the report says regarding custom, stability of names, etc. is borrowed from Articles 3 and 4 of the Paris Code that was not invented by Kuntze, and does not pay any compliment to his skill. Dare I say *more Kuntzeano*, that the gloomy doctor possesses "un véritable génie pour tordre et défigurer les choses les plus simples"? God forbid! I confine myself to noting the black ingratitude shown toward M. Ascherson, who, before the congressionists of Genoa, wished to draw a veil over the disastrous appreciation of the *Revisio* by the Viennese botanists, and relieved me from making any translation. But M. Kuntze dotes on eulogies and treats as *compères filous* (*sic*) those who hide them from the world. He finds eulogies in the Viennese reports as he finds them in M. Poirault's bibliographies. It is a little weakness that one must allow to great men.

And, *à propos* of great men, granted the prodigious, the phenomenal aptitude for work of which Otto Kuntze has given us such startling proof, is it not a pity that this force should expend itself on barren argument over words and names? Who can M. Kuntze hope to convince with his intemperate language? Is it not better to persuade than to abuse? Once more, it is a disease, and M. Kuntze gives fair game to his contradictors (pardon! to his persecutors), by lightheartedly "suiciding" his *Revisio*, which contains *passim* some excellent things. He must take care of his temper, improve his naughty humour.

The atrabiliar temperament, complicated with ideas of persecution, only demands attentive hygiene, exercise, fresh air, a cold bath on rising, not massage but gentle aperients, very little spice and nomenclature, much bicycle, a bit of fishing with a worm, walks (vasculum on back) through fields and woods. Twenty-five thousand hours' sedentary hard work, for seven years, to make columns and columns, with the aid of dictionaries, of names, *nuda* and *semi-nuda*, and accepted, and to blow a gigantic soap-bubble which the first critical pin-prick bursts piteously—what health could resist it? And this, properly speaking, is not botany.

"Botany," says a worthy American colleague, "has for its aims the study of plants, and not that of their labels." Botany is a healthy and delightful occupation—it softens manners. Nomenclature, in the method of Kuntze, reduces them to savagery. This parasitic science,

[1] And not *sous réserve des principes* (*unter principiellem Vorbehalt*) as Kuntze falsely quoted in *Rev. g. pl.* iii. (2), p. 67.

this *Pulex irritans* of botany, tends to bring upon us the laughter of the public. If we cut one another's throats over *Chocho, Gansbium, Belutta-Pipi, Peckifungus, Biscoigneauxia*, or *Bisaschersonia*, our reputation as scientific or serious men is gone. "Féroces comme Vadius et vaniteux comme Trissotin," is already scored to our account *à propos* of "Trifolium." We must put the drag on.

Study and flowers! especially flowers!

[Then follows "Note supplémentaire (altérations grossières du code de Paris par Otto Kuntze," but these being pure technicalities we must refer specialists to the original pamphlet, which, though it is not stated, seems to be an extract from some periodical.]

# Regeneration: Facts and Interpretations.

By Professor August Weismann.

My original intention in this essay[1] was simply to give an account of some observations, recently published but probably not yet widely known, which seemed to me to throw fresh light on the phenomena of regeneration. It became necessary, however, to interweave with the narrative some general remarks, in order to show on the one hand how these and other additions to the data of our science harmonise with the views I have previously developed, and on the other how these views must be modified in order to bring them into accord with the facts of the case, as we now know them.

We will first consider experiments made by Edmond Bordage,[2] Director of the Natural History Museum on the island of Réunion (Bourbon), with regard to the regeneration of the beak in birds. These deserve general interest, since they show that what has hitherto been regarded as a rare exception is really the rule, and since thus one argument against the interpretation of regeneration as an adaptive phenomenon falls to the ground.

In supporting this interpretation of regeneration I have previously[3] expressed the opinion that the regenerative power of a part is to be considered, not as a direct and necessary expression of the nature of the organism, but rather as a capability which, though it may be absent, is found wherever it is necessary in the interests of species-preservation. The capability of a part for regeneration seemed to me to depend, *ceteris paribus*, on whether the part was frequently liable to be lost in the ordinary course of life, and also on its relative biological importance for the animal. The weak and almost rudimentary limbs of *Siren* and *Proteus*, which swim in snake-like fashion, are not replaced

[1] Translation revised by Prof. W. N. Parker.

[2] *Compt. Rend. Soc. Biol.* Paris, July 9, 1898.

[3] "Das Keimplasma, eine Theorie der Vererbung," Jena, 1892. (English Ed., "The Germ-Plasm, a Theory of Heredity," London, 1893.) The idea there elaborated had been previously more or less distinctly suggested from various quarters. Thus even Réaumur pointed out that the power of regeneration was especially characteristic of animals whose brittle body was frequently liable to risk of breakage, and also of those, like earthworms, which are liable to be partially devoured by others ("Mem. pour servir à l'hist. insectes," Paris, 1738). In modern times, Lessona and Darwin have developed the same idea.

when amputated, but the reverse is true in regard to the gills, which, in these forms, are not infrequently bitten off. The latter are physiologically important organs, but this can no longer be said of the former. On the other hand, the strong and biologically indispensable limbs of the less elongated newt (*Triton*) have a high regenerative capacity.

The fact that internal organs not naturally exposed to mutilation are not regenerated in animals which show great regenerative power as regards external structures is in harmony with this view. Halved lungs in *Triton* do not grow again, but simply close up, and I can now add to this the fact that the oviduct and the vas deferens are neither renewed nor even elongated by fresh growth if a portion of them be excised. The experience of pathologists is also in agreement with this, for they report that they have never seen the regeneration of an experimentally excised lobe of liver or kidney in any animal, but only an increased function in the remainder of the organ—in short, there is no morphological regeneration but only a physiological compensation. On the authority of my renowned colleague in pathological anatomy, Ernst Ziegler,[1] I may add that, in man, the power of regeneration is possessed only by such tissues and portions of tissues as are subject, in the normal course of life, to continual or periodical wearing out. Such are, the mucous membrane of the intestinal tract, the epidermis of the skin, the whole mucous membrane of the uterus with its glands, etc. To these must be added certain tissues which are very frequently but not regularly or periodically liable to injury, and are at the same time of great biological importance. To this category the cutis belongs. In it, however, as is well known, complete histological renewal does not take place, but only a partial reparation by means of cicatrisation. The scar-tissue—which is also formed from connective tissue, but contains no skin-glands, and has a different arrangement of the connective-tissue strands—is well adapted to the closing of small skin wounds, and quite sufficient to re-establish in approximate integrity the important protective adjustments of the cutis. Regeneration never occurs, on the other hand, either in nerve-cells of any kind whatever, or in the supporting (neuroglia) cells of the brain, notwithstanding their great functional importance. All these facts are in harmony with the theory suggested, that the power of regeneration possessed by an animal or a part of an animal is regulated by adaptation to the frequency of loss, and to the extent of the damage caused by the loss. Hitherto, however, it seemed impossible to reconcile with this proposition the case communicated by Kennel,[2] to which I have already elsewhere called attention,[3] of the "stork the

[1] E. Ziegler, "Lehrbuch der allgemeinen Pathologie und der pathologischen Anatomie," Jena 1895. But the above statements are based on an unpublished lecture delivered by Ziegler in 1898 before the "Naturforschende Gesellschaft" in Freiburg-i-Br.

[2] Kennel, "Ueber Theilung und Knospung der Thiere," p. 18. Dorpat, 1888.

[3] "Keimplasma." (Eng. Ed. p. 125.)

upper beak of which was accidentally broken off in the middle, and the lower one being sawn off to the same length, both were completely regenerated."

Until quite recently there seemed no warrant for the assumption that the breaking of the beak in birds was of frequent occurrence, as no other similar observations were recorded. This case, therefore, seemed to indicate that the capacity for regeneration in a part does not depend on special adaptation to a great liability to loss, combined with high biological value of the organ, but rather that it is due to a general adaptation,—to a power of regeneration possessed by the whole organism which, to a certain extent, may come into operation in any part in which loss occurs, even though the loss takes place quite exceptionally.

This apparent contradiction of the adaptation-theory of regeneration is now set aside by the observations of Bordage, who observed that, in the island of Réunion, where the sport of cock-fighting is popular, injuries to the beaks of the cocks are of frequent occurrence, and are regularly followed by regeneration. Bordage found that, as the result of a fight, the beaks of the cocks were often damaged, but that afterwards complete regeneration took place. The injuries were partly inflicted by blows with the beak, partly by "un terrible coup de patte," but they usually affected at most only the terminal third of one or both jaws,—"that part of the upper mandible which represents the premaxillae or unpaired inter-maxillary resulting from the fusion of the premaxillae, and of the lower mandible the triangular portion formed by the fusion of the two bones at their extremity." These parts of the beak may be entirely broken off, when regeneration takes place, and both the bones and their horny covering are renewed.

In rare cases, the injuries to the lower mandible are so great that it is broken and hangs down, and has to be artificially fixed in its place. In such a case, of course, the animal cannot pick up its food, and has to be artificially fed, for the regeneration of the lower mandible requires two or three months.

Although these observations refer only to cock-fighting instigated by man, they have, as the author rightly points out, a wider bearing. It is well known that the males of numerous birds fight fiercely with one another in the breeding-season, the beak being naturally the chief weapon, and that this is true of storks was known to Brehm, who says that jealousy often leads to fatal combats.

The case described by Kennel can therefore no longer be regarded as evidence against the view of the adaptive nature of regeneration, and thus the one observation which gave ground for referring regeneration in a highly-differentiated animal to a *general* power of regeneration loses its force. In recent years, it is true, the renewal of the artificially removed lens of the eye in *Triton* has been cited in this connection; but this is hardly justifiable. I, at least, must own to being surprised

that the argument based on this case remained unchallenged. From Calucci's experiment of extracting the lens in *Triton*, which resulted, as was expected, in regeneration, G. Wolff[1] inferred that there was here "a new adaptiveness appearing for the first time," since it is "impossible that regeneration can take place in accordance with the inherited mode of its ontogenetic origin." The latter conclusion is indeed correct, but the former one does not follow from it, and it seems to me quite out of the question to bring forward this case as contradictory to the view that regeneration depends upon a special inherited adaptation. It will, of course, be readily admitted that newts, in their natural conditions of life, are not liable to excision of the lens, but it does not necessarily follow that the lens in these amphibians cannot be adapted for regeneration, for at any rate it may be lost along with other portions of the eye. As far back as 1781, Bonnet and Blumenbach showed that the eye of newts would renew itself completely even when all of it except a small portion (according to Blumenbach, as little as one-fifth of the bulb) was extirpated, and the more recent observations of Philippeaux (1880) have confirmed these conclusions. This regeneration certainly appears to us especially remarkable, since it necessarily presupposes quite a different mode of formation of the eye from that which takes place in the embryo. Leaving out of account altogether the question as to whether we are or are not in a position to form a theoretical conception of the phyletic origin and the nature of this process, it is at any rate certain that some sort of mechanism for the regeneration of the eye exists. But if this is true *for the whole eye*, why should we be surprised that a *part* of it, the lens alone, may be regenerated. A single toe of the newt's foot, if cut off, will grow again just as well as a whole leg.

There is no doubt, however, that the eye of *Triton* is susceptible of injury, and that it is frequently injured in natural conditions. Even newts fight among themselves, and I have elsewhere shown[2] that these animals, at anyrate in the breeding-season, ferociously attack and injure one another. I once put a number of newts together for a short time in a small empty glass, and I then saw that the animals attacked each other furiously, biting and struggling pertinaciously. "Several times one seized another by the lower jaw and tugged at it so violently that it would have been torn out if I had not forcibly separated the animals." From this I concluded that "the loss of part of a jaw or eye may therefore not infrequently occur in the natural state," and I may now add that the newt is liable to attacks by water-beetles of the family Dytiscidae. I was for a time uncertain whether the powerful poison which newts, like toads, secrete from the skin was any protection against such attacks, but this is not the case. Large Dytiscidae attack the newts whenever they can find no other food, and they eat away

[1] *Arch. Entwickelungsmechanik*, Bd. i. p. 380.
[2] "Keimplasma," p. 167. (Eng. Ed. p 125.)

their flesh to such an extent that the newts die. With small Dytiscidae I have as yet made no experiments, but it is unlikely that the poison serves as a deterrent to them either, and, since they are unable to kill the newts, they are the more likely to injure and eat away certain portions, such as the eye, with their sharp-pointed jaws. A trustworthy observer informs me that the larva of *Dytiscus marginalis* seems to go to work on a certain system, for it tries to get at the newt from above, and to attack it on the back just behind the head. Observations specially directed to the point may perhaps determine whether injuries to the eye take place. But even then we should have no *proof* that the regeneration of the eye is to be regarded as a case of adaptation of the organ to its frequently occurring mutilation. In such cases, however, formal proof is hardly attainable; it must always be a question of probabilities. Who, for instance, would attempt to determine how often the beak of the stork is injured? And, even if this were done say for ten years, over a comparatively large area of the bird's habitat, we should be confronted with the wider and unanswerable question, as to how often such injuries must occur in order that Nature may be prompted to effect, by means of selection, or in some other way, a mechanism of regeneration for the beak. But the mechanism in this case has been established, and for internal organs such as the lungs, oviduct, and vas deferens of newts it has not; there is therefore a greater probability in the conclusion that regeneration is regulated by adaptation, than in the inference, from the renewal of the lens in *Triton*, of the presence of a general power of regeneration,—"an adaptiveness not inherited but primary." It will, moreover, be apparent farther on that we have good grounds for assuming that an apparatus for regeneration once established degenerates with exceeding slowness when it has become superfluous. It is thus even conceivable that this apparatus may still persist although no longer necessary in the present conditions of the newt's life.

That such an apparatus is certainly present in this case is apparent even from the fact, in regard to which all past and present experimenters are agreed, that the eye of the newt is never regenerated if it be completely excised, and that therefore it is not any kind of cells, but definite cells belonging to the organ itself, which institute the regeneration, as is true in the regeneration of the intestinal canal in pupahood. As I showed in 1864[1] in the case of *Musca*, the intestine during this phase of life breaks down by histolysis and is immediately reconstructed. In 1888 Kowalewsky[2] and van Rees[3] were able to demonstrate in the same insect, and C. Rengel in 1896 in a beetle, that the new formation had its starting-point in special cells, fairly equally distributed throughout the mucous membrane, which did not break down during pupahood, but on the contrary increased rapidly,

[1] *Zeitschr. wiss. Zool.* 1864, Bd. xiv.
[2] *Op. cit.* 1888, Bd. xlv.
[3] *Zool. Jahrb.* 1888, iii.

and gave rise to the new mucous membrane. These "primitive cells of the epithelium of the mid-gut in the imago," as Rengel[1] calls them, thus form in this case the distinctly demonstrable regeneration-apparatus of the epithelial wall, and we may assume the existence of a similar regeneration-apparatus in the eye of the newt.

The results of an investigation bearing specially on the question of the adaptive nature of regeneration have been recently published by T. H. Morgan,[2] and these results are undoubtedly of value, although I find myself unable to agree with his interpretation of them. He selected the hermit-crab (*Pagurus*) as the subject of his investigations, and this was certainly a very suitable choice, since the experimenter has here at his disposal a series of appendages which, on the one hand, are very different in their biological value, and on the other are exposed to the danger of loss in widely varying degrees. Examination of freshly-caught animals showed that, out of a hundred hermit-crabs, nine lacked one of the three anterior legs, while none were without the small fourth and fifth, which probably serve to fix and support the animal within its gasteropod-shell. This result is only what might be expected when we bear in mind that the hermit-crab withdraws into its dwelling-place with great rapidity when danger threatens, and that the fourth and fifth legs must be the first to reach the shelter of the shell. In any case, these limbs are unlikely ever to be the aim of an attack, which is probably always directed against the three larger and more exposed limbs in front of them. As the posterior part of the body remains entirely within the shell, the abdominal appendages are perfectly protected, and could be injured only during a change of dwelling, or if an attempt is made to draw the animal forcibly out of its shelter. Accordingly Morgan found that only one out of a hundred specimens lacked the second or third abdominal appendage, and there is still the possibility that this defect was congenital, and not due to injury. The problem these experiments were intended to solve was this: *Is the power of regeneration graduated according to the probability of loss?* Are those limbs most easily and most frequently regenerated which are most frequently injured? and are those which are never injured, perhaps never regenerated at all?

Experiments in cutting off the various limbs showed that they were all capable of regeneration, though they did not all grow again equally often, the anterior abdominal appendages renewing themselves less frequently than the thoracic limbs, though even these did not become renewed in every instance. I need not go into further details; it is sufficient, as far as the main question is concerned, to know that all the limbs possess the power of regeneration, those most liable to injury and those naturally well protected alike. The relative biological importance of the different limbs in *Pagurus*, too, does not seem to be

[1] *Zeitschr. wiss. Zool.* 1896, Bd. lxxii.

[2] "Regeneration and liability to injury," *Zool. Bull.* Boston, 1898, vol. i. No. 6.

associated with any difference in the capacity for regeneration, since the almost rudimentary first pair of appendages on the abdomen of the male were renewed quite as often as those on the abdomen of the female, although these last fulfil the important function of egg-carrying.

These facts appear to tell very decidedly against the conception of regeneration as a capacity regulated by adaptation, and the author interprets his results in this way, and sums up as follows: "*There is no relation between the frequency of loss and the regenerative capacity of a part:* those who believe that there is such a relation overlook an important part of the problem. Even if it had been found that the parts most susceptible of injury were those most capable of regeneration, it would not follow that this depended on so-called Natural Selection. To assume this is to overlook the fact that unless these animals had had from the beginning the power of replacing lost parts they could not have survived at all, but would of necessity have become extinct." [1]

These are astonishing views. One might just as well say that it is impossible for Natural Selection to have brought about any change in an important part or character in the course of phylogeny, because the particular variation necessary to life must have been present from the first, since otherwise the species must have died out! It seems not to have occurred to Morgan that the changes in the structure of a species may have kept pace with the changes in the conditions of its life—yet this is a presupposition of the hypothesis of Natural Selection, and is indeed its *conditio sine qua non.* Hermit-crabs have certainly possessed the power of regeneration "from the beginning," but may they not have inherited it from their ancestors, the long-tailed forms, which possess it to this day and have need of it for all their appendages since all are liable to injury? And cannot, nay, must not, these in their turn have inherited it from their ancestors, the sessile-eyed crustaceans, and so on through the whole crustacean-pedigree back to the unknown annelid-like ancestors of the class? But doubtless even these possessed in a certain degree—probably even in a higher degree—the power of regeneration, from which that of the oldest crustaceans must have originated, and become localised and transformed. We know that the lower worms have quite as high a regenerative power, extending to all their parts, as the lower Coelentera or polypes for instance. It seems almost as if Morgan ascribed to me the view that the capacity for regeneration must be built up anew for each species—must be inscribed so to speak on a *tabula rasa:* my view, however, is that here, as in all transformations, Nature started with what was already present, and by modifying it brought about adaptation to new conditions. The assumed general power of regeneration in the lowly ancestors of the crustaceans

[1] This quotation is translated from the German. We have been unable, at the moment, to procure a copy of Mr. Morgan's paper.—[Tr.]

would thus gradually have adapted itself to the changes in the body, and to the new conditions resulting from these changes as well as from other causes; it would have become localised and specialised. In these animals no portion of the body can now give rise to a whole animal, but the parts most exposed to injury, the appendages, have retained the transmitted power of regeneration, and localised it in certain parts, and in relation to definite stimuli. As in the course of time the appendages of the different body-segments became more widely differentiated in adaptation to different functions—giving rise to antennae, jaws, walking-legs, or swimmerets—the predisposition to regeneration in certain parts of the body slowly varied also; and thus, not indeed at the same rate, but not lagging very far behind, the adaptation of the capacity for regeneration followed the adaptation of a limb to a new function.

Any one who has followed me in my attempt to formulate a theory of regeneration will admit that all this thoroughly agrees with the principles on which it is based, and will also see that it harmonises with the theory of Natural Selection much better than any conception of regeneration which has been or could be brought forward. It is clear, for instance, that the variation of the regeneration-"Anlage" must always take place *much more slowly* than that of the part itself—in this case the appendage. For since Natural Selection consists in a sifting out of the most fit, the rapidity with which it secures its result—namely, transformation—must depend, *ceteris paribus*, on the number of individuals of a species which are selected in relation to the part in process of transformation. If in a species of which one million individuals are living at the same time, nine-tenths accidentally perish, there will remain only 100,000 for the selection of the 1000 which we will assume to form the normal strength of the species. The greater the number among these 100,000 possessing the favourable variation, the higher will be the number of the normally surviving 1000 possessing it, and the more rapid will be the progress of the favourable variation.

If, however, it be a question of the variation of a regeneration-"Anlage," selection of its favourable variations will not operate among the 100,000 individuals which chance has spared, but only among those of them which, in the course of their life, lose the limb in question and thus are in the position of having to renew it, well or ill. Assuming that this is the case with ten per cent, selection towards the improvement of the regeneration-apparatus would only operate upon 1000 individuals, and the process of transformation of the regeneration-"Anlage" would go forward much more slowly than that of the transformation of the limb itself.

Another fact must be noticed in the same connection. It has already been observed in a series of cases, and would probably be found to occur in many more if thorough investigation were made for

the purpose, that appendages are sometimes regenerated, not in the present modern form but, in all probability, on an older type. In fact, reversion to an ancestral type occurs: *i.e.* the regeneration-"Anlage" has not yet quite reached the same stage as the variation of the part itself. Thus the appendages of *Blatta*, although growing again satisfactorily, are then, according to Bateson, provided with a four-jointed instead of a five-jointed tarsus, and the same thing has been observed by Bordage in the Phasmidae. Fritz Müller also records that the long-clawed forceps of a Brazilian shrimp, *Atyoida potimirim*, is replaced by the older short-fingered type of forceps, similar to that of the allied genus *Caridina*. Such cases might, of course, be regarded as showing an insufficiency of regenerative power to replace the lost limb perfectly; but Barfurth has shown that on removal of the four-fingered fore-limb of the axolotl a five-fingered atavistic limb grows in its place. In face of such facts the hypothesis of a general power of regeneration, which restores the damaged "life crystal" to its integrity, is obviously untenable. Here the "crystal" is restored after an older pattern, and one cannot see why this should be so, since the whole has been no more altered than a dodecahedral crystal would be if the point were broken off. If, in that case, the missing portion were suddenly to be re-crystallised on another "system," it could only be because the elements from which the restoration started were, not the remaining whole, but particular parts belonging to another "system," or—to translate into the language of biology—because at the injured places there were regeneration-"Anlagen" of earlier origin which came into play as the result of the injury. The existence of "Anlagen" is thereby proved.

It has been said that to explain such facts by reversion is to give no explanation at all; and that such an interpretation, though resulting in ranging the facts under a definite category of phenomena, in no way elucidates the cause which, after the loss of an eye, induces the appearance "of a different type of ancestral-organ. This, however, is just what we want to know" (Herbst). For my own part, I think it is better not to want to know too much all at once. It may be that some day we shall attain to a knowledge of the actual physico-chemical processes which condition development; but, in the meantime, we are many times the distance to Sirius short of that knowledge, and we shall do well to content ourselves with first striving after nearer goals. Even the simple recognition that we have here to do with a case of atavism—that is, of reversion to an ancestral form—is a creditable piece of knowledge, since it at least enables us to understand why the lost eye of the crab is not replaced by, say, a tuft of feathers, or a monkey's tail. Perhaps, however, the idea here expressed, that the regeneration-"Anlage" lags behind the phyletic transformation of the part itself, may carry us a step farther, always presupposing that we do not regard the principle of Natural Selection as "the greatest error of the century" (Driesch).

Let us now return to Morgan's results with the hermit-crab, in order to attempt an explanation of the fact that all the appendages of this animal have remained capable of regeneration—even those which are protected by the shell in which it lives, as well as the abdominal appendages of the male, which are presumably rudimentary and have no function to fulfil.

The cause of this seems to me to be the same as in the case I have just discussed, viz. the lagging of the regeneration-"Anlage" behind the progressive transformation of the limb—the fact that variation, even if it be retrogressive, goes on much more slowly in the regeneration-"Anlage" than in the limb itself. The predisposition to regeneration is useless in the case of limbs which are completely concealed and therefore not liable to injury, as well as in the case of non-functional appendages: but it cannot be in any way harmful, and therefore will not be gradually got rid of by an active negative process of selection—as are the abdominal appendages on the right side which are directed towards the axis of the spiral shell—but by that slow process of disappearance of parts which have become useless but are not harmful, which I refer to the operation of germinal selection under the influence of Panmixia. I hope to discuss this process more fully elsewhere, and, for our present purpose, it is not essential to have before us a clear theoretical statement of the gradual degeneration of useless parts and of the regeneration-"Anlagen." If it be certain that this gradual down-sinking and degeneration takes place *with extreme slowness*, we need not wonder that the predisposition to regeneration, which was for ages indispensable to the life of the animals, should still persist long after it has become useless and superfluous. From a phylogenetic point of view, we are justified in saying that the whole group of hermit-crabs came into existence only recently, that is, since the end of the Cretaceous period. In any case, we know that a species of Urodele (*Proteus*) has lived an incomparably longer time in the caves of Krain, and we need not wonder that in this hermitage it has lost the power of regeneration in limbs and tail, while the hermit-crabs still possess it in their abdominal appendages.

There is yet another group of phenomena which stamp the power of regeneration as an adaptation on the basis of inherited preliminary conditions, viz. the self-amputation of limbs, a process which has repeatedly been studied of late years. This was described by M'Culloch in 1826, later by Goodsir, and in recent years by Frédéricq and others. Bordage has also written about the so-called "autotomy" of leaf-insects (Phasmidae), in regard to which he established the fact, mentioned above, that young specimens of *Monandroptera inuncans* renew the tarsi after amputation with only four joints instead of five. Bordage[1] has taken advantage of his recent sojourn on the island of Réunion to

[1] "Sur les localisations des surfaces de régénération chez les Phasmides," *Compt. Rend. Soc. Biol.* Paris, 1898.

investigate the process of regeneration in the Phasmidae more fully, and he found that artificially-caused defects were made good only from particular regions. Regeneration from the cut end takes place only if the tarsus or the lower third of the tibia be cut off. If the limb be cut through higher up, the remaining portion of the leg is thrown off, and regeneration starts from the point of fusion of the trochanter with the femur, these parts not being connected by a joint as in other insects, but forming a thin suture at their union, and it is there that the breaking off of the limb takes place. The same state of things prevails among crabs and shrimps, in which M'Culloch has described the mechanism which brings about self-amputation of a limb when certain stimuli, such as compression or amputation, are brought to bear on it. At this point regeneration takes place, and here, therefore, the power of regeneration is localised. Both the capacity for autotomy and the power of regeneration limited to certain definite regions obviously imply not "primary adaptiveness" (*Zweckmässigkeiten*), but secondary arrangements, adaptations to perfectly definite and special conditions.

In order to learn something of these conditions in the case of the Phasmidae, Bordage instituted various experiments. He placed the insects with some birds, and observed that the latter—as indeed was to be expected—never merely injure the insects but invariably kill them outright by pecking at them with their bills. This is particularly true of the king-fisher of that region, *Acridotheres tristis*, which is a great destroyer of the grasshoppers and Phasmidae.

Birds, therefore, are never the cause of autotomy and regeneration of a limb; they are simply among the eliminators of these insects, not among the moulders of their regeneration-arrangements. But it is otherwise with small lizards. These (*e.g.*, *Calotes versicolor*) often seize the Phasmid by the body and swallow it at once, and this happens particularly often to the larva. But when an insect is full grown and its assailant is relatively small, the latter usually only succeeds in seizing its victim by one of the long legs, and then it tries to work its way gradually upwards with its mouth so as to get hold of the body itself. It does not bite very deeply, but presses the legs with just sufficient force to prevent the prey escaping, the insect meanwhile resisting by clinging convulsively to the nearest object. The stimulus supplied by the compressing teeth and the forcible pulling at the leg may cause the breaking off of the latter at the suture, and if the insect then lets itself fall from the twig on which it is sitting, it is saved. But, according to Bordage, in most cases it does not do this, but tries to run away, and is then easily recaptured and overpowered by the *Calotes*.

It happens sometimes, though not often, that the lizard catches hold of the leg by the tip only, and breaks it off, and thereupon regeneration ensues as already described, without the preliminary throwing off of the whole leg, which occurs only in response to

stimulus to the femur—when it is pinched, crushed, or cut through the middle: only in this region does autotomy occur.

Bordage states that the act of emerging from the egg may result in the loss of the tarsi and their subsequent regeneration, for it frequently happens that the insect does not succeed in freeing one of its legs from the egg-shell, which it is obliged to drag about to the great hindrance of its progress, until it is finally stopped entirely by the catching of the shell on some object. In such a case the insect struggles with all its might to free itself, and in doing so not infrequently loses the tarsus.

Something similar takes place during the process of ecdysis, for it frequently happens that the insect remains entangled in the old skin. Of a hundred Phasmids nine died in this way during moulting, while twenty-two tore themselves free with the loss of one or more legs, and only sixty-nine survived the process without loss.

Obviously, then, the power of autotomy with subsequent regeneration is here a frequently-used and advantageous arrangement; but I cannot agree with Bordage in regarding the pulling and struggling of the insect in the effort to free itself from the encumbering skin as the *directly* operating cause of the fusion of femur and trochanter which gives rise to the breaking-point adapted to autotomy. Even if this pulling and struggling lasted for a whole day, it is extremely doubtful that it could induce anchylosis of the joint; and if it could, it would be impossible that the anchylosis should be transmitted, and that it should thus form the basis of a general and transmissible fusion of the joint; for how could a variation of the joint influence the eggs or sperm-cells of the insects?

The facts communicated by Bordage are especially of value, because they show that it is not only the enemies of the insect in question that have a part to play in the development of the tendency to autotomy and regeneration, as has hitherto been thought: other events are also concerned which are of general occurrence and are repeated several times in the ordinary course of life, viz. the moultings. It is not impossible that the ecdyses of crustaceans operate in a similar way, but no precise statement on this point can be made without fresh investigations.

With regard to the phyletic origin of the adaptations in question, Bordage supposes that the capacity for regenerating the tarsus, the lower end of the tibia, and the whole leg from the suture has been derived from autotomy, which, again, he believes to have been brought about by persecution on the part of the saurians and batrachians of primeval times (Stegocephala). I prefer, on the contrary, to regard the power of regeneration as the older capacity, and autotomy as derived from it; for I doubt whether any part could regain the power of regeneration if it, or the parts from which it is phyletically descended, had once lost this power. In my opinion, therefore, the

legs of insects must have been capable of regeneration from the first, but may have entirely or partially lost this power in many groups, or have so modified it that it remained only at certain definite regions, as in the Phasmidae. Were the power of regeneration once lost it is scarcely possible that it should ever have been regained.

Autotomy, on the other hand, is an adaptation which might, phylogenetically speaking, arise anew at any time, where necessary, assuming that the existing parts were capable of varying and adapting themselves to that end. The existence of an autotomic adaptation of an analogous though not quite identical kind in insects and in crabs shows that the adaptation could take place in both classes, and the presence of such an adaptation in relation to the three anterior thoracic legs of *Pagurus*, together with its absence in relation to the fourth and fifth legs (Morgan), is further evidence of the comparatively recent introduction of autotomy and the much older origin of regeneration; for the fourth and fifth pairs of legs possess the power of regeneration.

The adaptation for autotomy once gained, the power of regeneration had of necessity to become localised; that is to say, the apparatus necessary for it had to be transferred to those parts at which alone the breaking off of the limb occurs in response to certain definite stimuli. In any case, however, both capacities are "*Einrichtungen*," that is, *adaptations of the organism to definite demands made upon it by the conditions of life*, and are not the outcome of primary qualities of the living substance. In regard to autotomy this is immediately clear because we know the reflex mechanism and the modification of the breaking-point upon which the adaptation depends, but in the case of regeneration we can in the meantime only guess at the mechanism which brings it about. That there is some special mechanism hardly admits of dispute.

In the beginning of the paper from which I have quoted above, Morgan casts a glance at my theory of regeneration as an adaptation-phenomenon, and not exactly a friendly glance either; for it seems to be the fashion nowadays among the younger investigators to look down with a certain lofty disdain upon so-called "explanations" which are based upon the selection-hypothesis. Morgan cannot believe that the chapter on regeneration in the "Germ-Plasm" will convince any one that the phenomena are "in any way" there explained, and he reaches a climax in the dictum, "it seems that the 'Natur-philosophie' is not yet dead." To that I can only answer: It is to be hoped that it is not, and that it never will be dead, for progress in our knowledge will always depend upon the philosophical treatment of known facts, since it is only in this way that we can set up new goals to guide our observations, and so find out new facts which will give us a deeper insight,—only in this way, too, can we recognise the importance of the facts which chance reveals to us. But if Morgan means by "Natur-philosophie" merely the degenerate varieties of philosophical Nature-

study as practised by Oken, Schelling, and others in the beginning of this century, one should surely recognise the difference that exists between this juggling with ideas and *a priori* interpretation and my attempts to gather all the facts together under a common point of view. The former are mere fantasies which will never establish anything, the latter are, to some extent, central points for scientific tasks which will yield to the investigator provisional assumptions to be corroborated or disproved: they may even give into his hand biological formulae or symbols which, though incapable of full explanation as yet, may with advantage be included in the consideration of certain problems.

When Oken, for instance, says "the contrasts in the solar-system between the planets and the solar bodies repeat themselves in plants and animals, and as light is the principle of movement, the animal has the power of independent movement as an advantage over the plant organisms which are fixed to the earth," no one will attempt to make anything of it, altogether apart from the fact that the *tertium comparationis* immobility, as we now know, is not peculiar either to the sun or to the plants. Again, Oken, with true prevision, stated that "everything organic has come from slime, and is nothing but slime in various forms; this primitive slime had its origin in the sea in the process of the development of the planets from inorganic material"; and then the primitive slime took the form of little vesicles, and "the whole organic world has as its basis an infinity of such vesicles." These are *a priori* interpretations of nature which by chance come very near the truth, but which, when they were put forward, could not in the smallest degree further the progress of knowledge, because no one then possessed the means of proving them right or wrong, or of deriving from them any deeper insight into the process of life. But, on the other hand, the "supposition" advanced in the "Germ-Plasm," that regeneration may be an adaptation-phenomenon and as such depends upon processes of selection, is not a final verdict but simply a hypothesis which may give a new lead for investigators to follow, as Morgan himself has done.

Conclusive results, of course, cannot always be obtained from a single series of experiments, for Nature's answer to our questions is not usually capable of only one interpretation, and criticism is often necessary in order that it may be read correctly. Thus Morgan's results do not afford a convincing refutation of my theory of the adaptive nature of regeneration; they have even, indeed, a bearing which tells decidedly in favour of my view. If the provisional conclusion that the almost rudimentary abdominal appendages are regenerated less frequently than the actively functional walking legs is corroborated, it, at any rate, will be intelligible from the standpoint of the selection-interpretation, but not from that of those who regard the organism as a crystal, and regeneration as an inevitable completion of the crystallisation. For if

no "Anlagen" be necessary to the occurrence of regeneration, there are none to disappear when regeneration becomes useless on account of the unimportance of an organ. It is true that Morgan does not state that this conclusion is reliable, and it will need a much larger number of experiments to prove it satisfactorily. A continuation of these experiments might also yield much in other directions. It would be necessary to observe the complete development of the regenerated abdominal appendages in order to be able to say definitely whether they were reproduced in the same form, or whether here too there was reversion to an older type. If the above conclusion be correct, *i.e.* if the phyletic variation of the regeneration-"Anlage" goes on more slowly than that of the transforming organ itself, we should expect, for instance, that in the regeneration of the last abdominal appendage, the present form—which serves as a holdfast—would be renewed after the pattern of the tail-fin of the Macrura. This is, of course, only a supposition, and it is perhaps more probable that in this case the regeneration-"Anlage" has followed more closely on the transformation of the organ, as it must be of importance that the holdfast should be renewed as such. Of course this argument would fall to the ground if it were shown that injury to this sixth abdominal appendage hardly ever takes place; for then there would be no reason for the progressive modification of the regeneration-"Anlage." But the statistics given are not nearly comprehensive enough to admit of any certain conclusions being drawn from them, and further experiments are much to be desired.

Let us now discuss another experiment made on frog-tadpoles, which, if it be corroborated, will be of fundamental importance to the question of regeneration. Before the first external indications of the hind-legs appeared, Esther Brynes[1] destroyed the whole region of the body-wall from which the legs grow with a hot needle, and observed that perfectly normal limbs were nevertheless formed. The experiments of Barfurth had previously shown that the legs of young tadpoles become regenerated, and had also established the fact, which seems to me very important, that this power of regeneration is rapidly lost as the tadpole grows. These experiments are quite in accordance with the theory of the localisation of the regeneration-"Anlage" at the base of the leg, since it must somehow be present at this stage. Esther Brynes's new experiment, on the other hand, tends to show that at an earlier stage there is also regeneration of the leg without any localised "Anlage," and that at this stage the limitation of the "Anlagen" to the cells of the mesoderm has not yet taken place.

The knowledge we have already acquired through the numerous and variously modified experiments with the blastomeres of the developing ovum here receives a new and very precise corroboration.

[1] On the regeneration of limbs in frogs after the extirpation of limb-rudiments," *Anat. Anzeig.* 1898, Bd. xv. No. 7.

Certain cells of the developing animal contain, to begin with, many "Anlagen," and which of these shall become active is decided by the position the cells take up in the organism, *i.e.* through the influences exerted upon them by the rest of the living parts of the animal.

All these phenomena, and also more especially that which Roux has called "post-generation," make it plain that my theory of ontogeny requires some modification. When it was put forward, two fundamental postulates for the explanation of ontogenetic differentiation occurred to me as possible—either that there is a regulated progressive breaking up of the "Anlagen"—masses contained in the germ-plasm into smaller and smaller groups; or that the collective "Anlagen" remain together in all the cells of the organism, but that each of them was responsive to a specific stimulus, which alone could excite it to activity; the former view may be described as a theory of dispersal, the latter as one of liberation. I inclined to the former, because, in face of the facts before us at that time, it seemed on the whole the more probable; and because I found myself in the position of being obliged to make a choice if I wished the applicability of my principles of heredity to be tested in relation to all aspects of the problem. I found myself in the position which the present Prime Minister of England, Lord Salisbury,[1] referred to in his Oxford address as one in which politicians found themselves often, but naturalists never—that of having to choose the lesser of two evils or uncertainties. The mere observer of facts certainly does not find himself in this position, but it is otherwise with the theorist who is confronted with the task of associating in idea the phenomena of a whole domain, filling up the gaps in the knowledge of the moment with probabilities, seizing the essentials of both, and combining them on a definite principle into a provisional theory. He must choose between greater and lesser probabilities if his theory is to be worked out at all, just as the politician must act, if the whole machinery of the State is not to come to a standstill.

This is by no means meant to indicate that I now consider my idea of a regular dispersal of the germ-plasm in the course of ontogeny incorrect; it rather appears to me a more indispensable assumption than ever, but I recognise that liberation (Auslösung) plays a larger part in ontogeny than I had hitherto believed, and that the assumed breaking up of the germ-plasm does not, at least in many cases, take place at the first division of the ovum, but later.

It is not my intention to enter more fully into this question here, but I should like to emphasise the fact that a modification of my theory of ontogeny can in no way react upon the theory of heredity in the strict sense. My ideas as to the mechanism of heredity would remain unaltered even were I to adopt a pure liberation-theory such as

[1] "Address by the Most Hon. the Marquis of Salisbury," *Report Brit. Assoc.* Oxford, 1894.

has been recently developed by Oscar Hertwig.[1] I still believe as firmly as ever that the germ-plasm is made up of determinants,—that is, of groups of vital particles, the presence of which in the germ-plasm conditions the formation of definite parts of the organism, and the variation of which modifies these parts alone. The conception of the germ-plasm as composed of ids is also in no way affected by the newer facts, and it still seems to me not only a fruitful but an imperative hypothesis.

As far as the theory of heredity is concerned, it is a matter of indifference whether the determinants of the germ-plasm remain together during ontogeny, and are only individually excited to activity by specific stimuli, or whether they are dispersed during ontogeny into smaller and smaller groups. Even in the latter case liberating stimuli are also indispensable, and it does not affect the real problems of heredity—the inheritance of functional modification, the mingling of parental characters, reversion to nearer or more remote ancestors, and so on—in what particular way the individual determinants are excited to activity, provided that their existence is established. That they do exist, I consider certain, because it follows as a logical necessity from the facts of inheritance and variation. Determinants must be present because the parts of the organism are capable of individual and transmissible variation, and this is only possible if there are in the germ-plasm living particles related to definite parts of the perfect organism; they must be present, not in the sense of the "Anlagen" of the old "evolution" or preformation theory, in which they were regarded as being themselves the part in question although only *in nuce*, but in the sense of *working, living particles which operate during the course of development in a definite manner, so that the part they have to determine is thereby produced.*

It is only the theory of ontogeny which is affected by the fresh facts, not that of heredity. The experimental determination of the prospective importance of the blastomeres which we owe to Roux, Driesch, Wilson, Chabry, and so many others, and the now indubitable process of post-generation as formulated by Roux, necessitate the assumption that the blastomeres commonly contain all the determinants within themselves, and that the cells of the external and middle germinal layers retain many of them for a long time. But it by no means follows that a pure liberation-theory (Auslösungs-Theorie), like that put forward by Oscar Hertwig, is correct. It is much more probable that at some period of development, at least in the case of higher animals, a limitation in number of the "Anlagen" takes place in the cells during ontogeny, and this may often go so far that they eventually only contain the one "Anlage" which determines their differentiation. I cannot agree with the statement in Hertwig's latest book, in which the supposition is made that all the cells in ontogeny contain the complete

[1] "Die Zelle und die Gewebe" (II.), Jena, 1898.

germ-plasm, and that it depends solely on the stimuli of position and the nature of the particular cell-body itself, which of the many "Anlagen" of the nuclear substance will undergo development. I may, indeed, have laid too little emphasis on the rôle of the liberating stimuli, and bestowed my attention too exclusively on the "Anlagen," but such an extreme liberation-theory as that of Hertwig seems to me to overshoot the mark on the other side.

If what should be formed at a particular part of the organism depended solely on the liberating stimuli, and the "Anlagen" were everywhere the same, the same organ would necessarily invariably be formed at any given place; and it would be impossible that—as has many times been observed—an antenna should grow out in place of an insect's leg (Wheeler), or a leg instead of an antenna (Kraatz), or that instead of an abdominal appendage in the common crab (*Cancer pagurus*) a walking leg should appear (Bethe), or that antennae should replace the amputated eye-stalks in shrimps (Herbst), or that four-jointed tarsi should replace five-jointed ones (Bateson, Bordage).

In regard to a few of these cases, one might seek to evade the conclusion by the unverifiable statement that the liberating stimuli are altered by the previous occurrence of injury. This may be so, but still no explanation follows thereupon as to why, for instance, a four-jointed tarsus is formed in place of a five-jointed one, since the four-jointed tarsi of the ancestors did not result from amputation. I do not see how it is here possible to avoid the assumption of different "Anlagen" which can be liberated by a similar stimulus; and thus that the "Anlage" is the main thing and the stimulus only of more general nature; the stimulus only determining that something is to be formed, the "Anlage" deciding what that something is.

I hope to discuss this question more fully on some other occasion, and therefore here only wished to indicate my position in regard to some of the facts and opinions which have come to light since the publication of the "Germ-Plasm." It will probably be necessary to make a compromise between the theory of dispersal and that of liberation, though it may not yet be possible even to sketch its outlines with certainty.

With regard, however, to the theory of regeneration, which is so closely dependent on that of ontogeny, the explanation of the renewal of a lost part is easy enough if we regard the complete germ-plasm as present in every cell; every "Anlage" could then come into operation at any place if we assume their specific liberation-stimuli to be present. But the effectiveness of the theory breaks down when we attempt to explain why, nevertheless, regeneration in so many cases does not take place;—why, for instance, internal and external parts of the same newt should behave so differently in this respect. If the injury supplies the specific liberation-stimulus, why does it not liberate the corresponding "Anlagen" in internal parts also? There seems to be

only one possible answer to this question, viz. because the "Anlagen" are not there.

The facts recently established certainly indicate that, at any rate among many animals, the embryo and the young organism possess many cells whose wealth of "Anlagen" can, under the determining guidance of the whole, bring about the regeneration of parts in different directions; but it seems to me equally certain that, as the organism approaches maturity, the number of such cells gradually decreases, and the potentiality of their "Anlagen" becomes gradually more restricted, until, finally, many cells are only able to reproduce their like. But how far this limiting of the "Anlagen" goes, and how far the cell contains inactive "Anlagen" ("Neben-Determinanten"), is obviously determined by the necessities of the case, and depends on adaptation; in so far, at least, it will hardly be disputed that the capacity for the regeneration of parts is regulated by adaptation. It is impossible to say whether we must not go much farther than this, and regard the power of the blastomeres to produce the whole as a very ancient but nevertheless secondary phenomenon, depending on adaptation to injury. But if we bear in mind how the differentiation between homoplastids and heteroplastids occurs in the Volvocidae, it is evident that the first separation of the germ-cells and the body-cells in this case cannot be referred to different liberating stimuli, but must consist in a separation of the "Anlagen"; the latter must therefore be the primary arrangement, and the equipment of the blastomeres with the whole germ-plasm only a secondary acquisition. We thus once more arrive at the view, adopted both by Roux and myself, of "reserve germ-plasm"—of supplementary germ-plasm in general,—with which Barfurth also agrees.

To this important point, too, I hope to return at a future time, when I intend to reply to the factual criticisms of my opponents. This short essay owes its origin to purely external reasons, and I have written it rather for the supporters than for the opponents of my views, and rather to show that the recently established facts harmonise well with the fundamental principles of my theory than to meet the objections of my antagonists in detail. To reply to the many invectives, the sarcasm and derision which have been showered upon me in such overflowing measure since the publication of the "Germ-Plasm" seems to me quite superfluous, for I regard such utterances as a not exactly desired, but yet a not altogether unsatisfactory sign, that the less noble emotions of human nature—envy, ill-will, and self-advertisement—have found cause to direct themselves against the results of my work.

To one reproach of a general and impersonal kind I should like to reply here, as it was first raised by one who is now dead, but has been urged again and again by those who wished to bring discredit on my views—the reproach that these views have no stability and that they change so ceaselessly that no one can know what my real opinion is. As a matter of fact, my ideas have changed on many points in the

course of the score of years which have almost elapsed since I first brought them forward, simply because I did not then know all that I know now. I may be permitted to give an example.

In 1885 I directed the attention of biologists to the importance of the "polar bodies" in relation to the theory of heredity. The persistence with which these bodies, in themselves apparently insignificant, appear throughout the whole animal kingdom seemed to me to indicate that they played an important physiological rôle which must be associated with their liberation from the egg, and in connection with the theory of the continuity of the germ-plasm—then only developed in rough outline—I suggested that they might have to do with the extrusion of the "ovogenetic" idioplasm, that is, with the removal of the nuclear substance determining the histological structure of the egg-cell.

This supposition of mine—my first hypothesis concerning the importance of the polar bodies in relation to the theory of heredity—was opposed to the opinions expressed earlier by Minot and E. van Beneden, who regarded the immature ovum as a hermaphrodite cell which became female only after the extrusion of its male elements, the polar bodies. Observation showed me that even parthenogenetic eggs form polar bodies, and consequently Minot's hypothesis was overthrown, while my own was strengthened. But further investigations by Blochmann and myself led to the discovery that parthenogenetic eggs only form one polar body, while those which require fertilisation always form two; and then, supported by E. van Beneden's fundamental discovery of the numerical equality of the paternal and maternal chromosomes in the fertilised egg, I modified my first hypothesis so far as to suggest that the "ovogenetic" idioplasm is expelled in *one* of the polar bodies, while the other implies a *reduction of the ancestral germ-plasms*, or *ids* as I now call them. The necessity of this process I attempted to prove, and postulated for its accomplishment a special mode of nuclear division, a *reducing division*, in which the usual longitudinal splitting of the chromosomes is suppressed, so that only half the number of these migrate to each daughter-cell. What was at the time only a theoretical conclusion merely indicated by the observations then on record and not deducible from or capable of proof by them, has been fully justified by later investigations. There *is* a reducing division.

In this second hypothesis, however, an error was also involved, for Boveri's[1] observations on the egg of *Ascaris* have shown that the chromosomes expelled from the ovum in the first polar body are not essentially different from those remaining in it, because in certain circumstances they are interchangeable with them. I therefore altered my second hypothesis to a third, according to which both divisions of the ripening ovum together imply a reduction in the number of *ids*,

[1] "Zellenstudien. Über das Verhalten der chromatischen Kernsubstanz bei der Bildung der Richtungskörper und bei der Befruchtung," *Jenaische Zeitschr. f. Naturwiss.* 1890, Bd. xxiv. p. 314.

and this position I still maintain. Previously to this, however, I had come to the conclusion, based upon the theory of the reducing division, that a corresponding process of reduction must take place in the male cell, and some time later this was actually demonstrated by Oscar Hertwig. It is well known that during the succeeding years many excellent observers have carried on this difficult investigation on the processes of reduction in male and female germ-cells, and I need only mention the names of Boveri, Henking, vom Rath, Rückert, Häcker, Korschelt, Ischikawa, and van der Stricht, and in the domain of botany, Strasburger, Ischikawa, Calkins, Belajeff, and Guignard. Thus the conclusion has been gradually more and more firmly established that in the sex-cells of animals, as well as in those of plants, a reduction of the number of chromosomes takes place, and that in the majority of carefully investigated cases this process is brought about in the same manner, that is, by a peculiar form of nuclear division which occurs only in this connection. Recently, too, increasingly reliable evidence has been furnished that a similar reducing nuclear division precedes conjugation in unicellular plants and animals (Maupas, R. Hertwig, Schaudinn, and others).[1] Isolated observations are always cropping up which seem to necessitate different interpretations; but we may expect, as has often happened before, that more thorough investigations will succeed in removing the discrepancies in some way or other. A review of the facts now established cannot but convince us that for a large series of organisms the reduction-hypothesis is already proved.

This advance in our knowledge was only possible on the basis of *changing* theories, one growing out of another, and it would be absurd to object to such modifications. It seems to me, on the contrary, that the great value of hypotheses and theories is just that they do bring about the necessity of modifying them. They form the indispensable ladder by which investigation descends step by step into the depths of the biological mine until it comes upon a new vein of ore—fresh guiding facts—when it pauses a while, exploring and exploiting them in all directions, and finally by proving them secures a basis of operations from which a new ladder may be sunk to lower depths.

One of my critics has compared my "theories" to "towns in the Far West," the houses of which are barely erected when they are taken down again to be rebuilt farther out in the unknown land. I accept the simile, provided it be not forgotten that the first house of the advancing pioneer must remain standing and in use for a time before the region beyond becomes accessible to further colonisation.

I admit that I have not only made unavoidable mistakes, but also some which might perhaps have been avoided, and that I have

[1] Cf. the excellent review and critical summary of facts recently given by Häcker (*Verh. Deutsche zool. Gesellsch.* Jena, 1898) under the title "Über vorbereitende Theilungsvorgänge bei Thieren und Pflanzen," p. 95.

occasionally over-estimated the bearing of a newly acquired item of knowledge in the joy at its discovery,—that is, I have looked at it from too one-sided a point of view. This was the case in my paper on the meaning of sexual reproduction, in which I represented this factor in individual variation as its real and main root, while it is, I now believe, only the indispensable means of mingling these variations and of their persistent renewal. It is immaterial whether I myself or one of my sharp-sighted critics was the first to discover this mistake; but in any case it gave me occasion to sink a new ladder into the mine of investigation, to bore for the deeper ore-stratum of the source of variation, and to find it in the minute local fluctuations in nutrition which strive to disturb the equilibrium of the system of determinants in the germ-plasm. In this way I arrived at the theory of "germinal selection," which, although recognised only by a few—publicly, I believe, only by Emery—as a justifiable hypothesis, will, I should like to believe, yield greater results in the future.

It has also been said that I have carried my theories much too far, and have lost myself in details and speculations. Ironical admiration has been bestowed on the courage which enabled me to apply my theories to particular problems, and which did not shrink from the many auxiliary hypotheses necessitated by applying the main theories to special questions. To speak thus is to overlook the fact that nothing but persistent pushing of a guiding hypothesis to its furthest limits leads to a recognition of its defects and weaknesses, and so paves the way for farther progress. It is quite easy to set up theoretical principles, to propound a "theory of heredity" or of ontogeny, if one confines oneself to elucidating only the most general phenomena. The test of the usefulness of theories lies in applying them to special phenomena; it then becomes apparent whether and where they touch improbabilities, and where new facts are required to improve them or to replace them by others. It is not the joy of "explaining everything," but the desire to see them tested on all sides and to give them freely into the hands of my critics, that has led me to apply my principles of explanation to particular problems and to carry them as far as possible. This has been well understood by Yves Delage[1]—by far the most objective of my hostile critics—in the following passage: "Enfin il faut savoir gré à Weismann d'avoir été jusqu'au bout des conséquences logiques de son système. *Il a tenu à tout expliquer*, et il n'a pas reculé devant la nécessité de compliquer sa conception fondamentale, si simple et cependant déjà si féconde, pour rendre compte des faits de bourgeonnement, de régénération, de polymorphisme, etc. Il aurait, en évitant d'en parler, comme tant d'autres, échappé à de graves objections; il a préféré les subir que de reculer devant les difficultés."

If it be asked why I did not keep silence altogether if many of

[1] "La structure du protoplasma et les théories sur l'hérédité," etc., Paris, 1895, p. 708.

my explanations were not satisfactory even to myself, I can only give the simple reason that the fundamental basis of the theory appeared to me useful, and a fully worked-out theory seemed necessary to raise new questions and lead to further progress. In the complicated domain of biology, and particularly in that of heredity, it is only through theorising that new questions are raised, and the path to the discovery of new facts pointed out.

The assumption of the existence of biophors and determinants seems to me as indispensable and quite as justifiable here as the assumption of atoms and molecules in the domain of chemistry. Modern philosophy would, indeed, be in the right in rejecting the idea of philosophical atoms as minute indivisible particles; the chemical atom, on the other hand, is so far real, as it is the expression of the relative weights in which the so-called elements combine together to form molecules. Without the symbol of chemical atoms and molecules, the whole marvellous deepening of chemical knowledge which the century now drawing to its close has brought about would have been impossible. In the same way, I believe that a deeper penetration into the problems of biology, or at least into that of heredity, will only be possible through the assumption of the symbols of biophors, of ids, and above all of determinants; and here also, when, with the help of these symbols, we formulate our questions to Nature, we shall be dealing, not with mere fanciful images, but with realities, just in the same sense in which chemical atoms and molecules are realities.

In my opinion, the belief that biology does not require the guidance of theory is a great and widespread error. Within a very limited range, an individual investigator may, without doubt, make progress in certain directions without any conscious guiding theories—that is, so to speak, with latent theories. In truth, however, if his research be at all far-reaching, it is always guided by hypotheses, the confirmation or refutation of which he seeks. But when an attempt is made to investigate a wide domain embracing many phenomena, unconscious assumptions are no longer sufficient; but definite and well-thought-out hypotheses belonging to a theoretical system are necessary. This is what I have attempted to supply with my theory of heredity,—not a structure calculated for eternity, but one which should serve as a solid nucleus, a crystallisation-point, for further research, and around which new acquisitions in knowledge should group themselves for a time. I do not believe that any one now living could think out and elaborate a theory of heredity which should withstand the course of time without requiring modification. The scientific general-staff is now so large, that no hypothesis can remain long untested; on the contrary, every new idea is at once seized hold of by an army of investigators who seek, if possible, to refute it, or at least to corroborate it. I do not understand how it can be seriously urged at such a time, as an objection to a theory in the complicated

field of Nature and Life, that it has been modified according to new discoveries, which it has itself in part brought about. That it should be capable of such modification without giving up its fundamental principles seems to me an advantage, and I trust that in the future, even more than in the past, the theory of the germ-plasm will do good service in widening our knowledge of heredity.

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# FRESH FACTS.

Geotaxis in Animals. Julia B. Platt. "On the Specific Gravity of *Spirostomum*, *Paramaecium*, and the Tadpole in relation to the Problem of Geotaxis," *Amer. Naturalist*, xxxiii. 1899, pp. 31-38. The tendency that some Infusorians have to collect near the surface of the water in which they live has been regarded as a reaction to the force of gravity,—a negative geotaxis. To test this Miss Platt has followed Dr. C. B. Davenport's suggestion of altering the density of the fluids. If F. Schwartz was right in his theory of negative geotaxis, the negatively geotactic organism should become positively geotactic in solutions of greater specific gravity than its own, supposing the animal to be normally heavier than water. Miss Platt determined the specific gravity of living *Spirostomum* and dead *Paramaecium*,—both about 1·017,—but got no positive results in regard to reaction to gravity as far as these Infusorians are concerned. "Small tadpoles that are negatively geotactic do not become positively geotactic when placed in solutions heavier than their own specific gravity, as one would expect were their upward motion in direct response to the action of gravity on the organism as a whole. These tadpoles show constant negative geotaxis in water, in a solution of their own specific gravity, and in heavier solutions."

Freshwater Protozoa. Robert Lauterborn. "Protozoen-Studien, IV. Theil. Flagellaten aus dem Gebiete des Oberrheins," *Zeitschr. wiss. Zool.* lxv. 1899, pp. 369-391, 2 pls. An impulse to the study of freshwater Protozoa should surely be found in the researches of Dr. Lauterborn, which show how little we know as yet of the numerous beautiful forms inhabiting ponds and lakes and river-beds. In a short time and within a limited area he discovered five new genera of Flagellata.

Regeneration in Tubifex. H. Haase. "Ueber Regenerationsvorgänge bei *Tubifex rivulorum* Lam. mit besonderer Berücksichtigung des Darmkanals und Nervensystems," *Zeitschr. wiss. Zool.* lxv. 1898, pp. 211-256, 2 pls. 11 figs. There is less regenerative capacity in *Tubifex rivulorum* than in many related Annelids. The posterior part is regrown more readily than the anterior end, where in fact never more than three segments are replaced. Except a small most anterior portion the regenerated fore-gut is endodermic, and not ectodermic as in normal development. On the other hand, the hind-gut is regenerated from the ectoderm just as it is formed in the embryo. The regenerated ventral nerve cord arises from an unpaired proliferation, but the supra-oesophageal ganglion has a paired origin which is, however, to begin with ventral in position.

Parthenogenesis in Stick-Insect. Stadelmann. "Ueber einen Fall von Parthenogenese bei *Bacillus rossius* F.," *SB. Ges. Natur. Freunde Berlin*, 1898, pp. 153-155. The occurrence of two successive parthenogenetic generations is recorded, thus confirming the previous observations of Dominique, Bolivar, and Krauss. The parthenogenetic ova developed into females only.

BIRD-CATCHING SPIDERS. R. I. POCOCK. "The Genus *Poecilotheria*: Its Habits, History, and Species," *Ann. Nat. Hist.* iii. 1899, pp. 82-96, 1 pl. This genus is a representative of that great and almost cosmopolitan group of spiders which was formerly included under the comprehensive title *Mygale*, which still persists in many books. The observations of Madame Merian and Mr. Bates have formed the slender foundation for the widespread and sensational belief that the staple article of food of these spiders consists of small birds. "As a matter of fact, there is no doubt that they feed almost entirely upon insects; but they will certainly also kill and devour any living animal they are powerful enough to overcome. The species of *Poecilotheria* are tree-living forms, and their colouring is adapted on the upper surface for protective concealment on bark overgrown with lichen and moss. The colour of the under side is startlingly different, is exposed when they rear themselves on their hind legs, and is probably of warning significance. They also possess a stridulating organ between the mandible and the palp. Eight species are described from Ceylon and S. India."

SELECTIVE ACTION OF FROST. T. D. A. COCKERELL. "Vernal Phenomena in the Arid Region," *Amer. Naturalist*, xxxiii. 1899, pp. 39-43. From observations on plants and bees made by himself and other workers at the Mesilla Park Experiment Station, New Mexico (altitude 3800 feet), Prof. Cockerell concludes that "throughout the arid region, where the sky is clear and the radiation great, the development of plants and insects is controlled largely by the distribution of frosts throughout the year. In the course of natural selection the native species (except such as are frost-proof) have learned not to appear or develop until the danger of frost is over." There is warmth enough, as is shown by the behaviour of introduced fruit-trees which rush into bloom and get nipped. The horticulturist's device must be to select late varieties which will escape the frosts.

DIMORPHISM. E. G. CONKLIN. "Environmental and Sexual Dimorphism in *Crepidula*," *Proc. Ac. Nat. Sci. Philadelphia*, 1898, pp. 435-444, 3 pls. In various species of this genus of sedentary gasteropods there is "environmental polymorphism," *i.e.* the form and colour differ according to the nature of the shells on which the animals live. In *Crepidula plana* there are dwarf and giant forms, and the difference in size is associated with a difference in the number, not in the size, of the component cells. The dwarfing is a "modification" and not inheritable. In the same species sexual dimorphism is also well marked, the average female being about fifteen times the size of the male.

DETECTION OF A ROBBER ANT. E. WASMANN. "*Lasius fuliginosus* als Raubameise," *Zool. Anzeig.* xxii. 1899, pp. 85-87. This glossy black wood-ant, which finds its usual food-supply in aphides, has not hitherto been known as a robber. But Wasmann has detected it in the act of plundering the nest of *Myrmica laevinodis* and carrying off the larvae and pupae. The observer does not believe, however, that we have here any hint as to the origin of a mixed colony. The captives are simply devoured.

# SOME NEW BOOKS.

## THE NATURALIST IN AUSTRALIA.

In the Australian Bush and on the Coast of the Coral Sea, being the Experiences and Observations of a Naturalist in Australia, New Guinea, and the Moluccas. By Dr. RICHARD SEMON. Roy. 8vo, pp. xvi. + 552, with 86 figures and 4 maps. London: Macmillan and Co., 1899. Price 21s. net.

It is right that this book, which was first published in Germany some three years ago (see our previous review of the original edition), should have been translated into English. It is professedly an account of an expedition destined to bring some phylogenetic problems nearer their solution; and as such it would be welcome to many English readers who are interested in the "living fossils" of the island continent. But it speaks to a wider class than those who trouble themselves with questions of affinities between the various groups of animals: every one with even a trace of the feelings of a naturalist and every one who concerns himself with the character and doings of primitive man is appealed to. Semon does not attempt to give details of the scientific results of his expedition, for these are appearing in his "Zoologische Forschungsreisen," but he gives a fascinating account of his observations of the habits of the animals that attracted him to the southern hemisphere, and he adds a lively sketch of the life and character both of the squatter and the "blackfellow."

The present writer has only recently returned from the region of Australia that is most fully described by Semon, and can heartily testify to the truthfulness of the book. Semon has succeeded in transferring to his pages much of the spirit of the bush; he gives no exaggerated account of life in the wilds, but, speaking with full appreciation of his facts, tells simply and happily the story of his adventures.

Much of the interest of such a work might have been lost in a translation, but such is not the case here; and the English edition is thoroughly readable. There are, it is true, a few obvious errors in the rendering, and a number of words and phrases with a distinctly foreign colouring; yet on the whole the version is most satisfactory.

The translation, as well as the method of the author, may be illustrated by the account that is given of the Australian opossum (*Trichosurus*). Semon figures this animal, and tells us about its skill in climbing and its nocturnal wanderings in search of insects, eggs, young birds, eucalyptus buds, and sour grapes. He describes the kind of trap with which he secured many specimens, and in connection with the observation that few of his victims had young in their uteri, he digresses to explain the bushman's belief that the young of marsupials are conceived on the teat. But when he comes to describe his experiences in shooting 'possums, he is even more discursive. Others in like position have felt the weird influence of the moon's rays shining through the

great gums, but surely no one has moralised on his feelings to such effect. "In Middle Europe," says Semon, "the moon during the summer months stands particularly low in the heavens, and remains but a short time visible. And still this is the very season when the balmy night air suffers us to stay out-of-doors to enjoy nocturnal scenery. In the tropics she stands nearly all night in the zenith just above us. . . . The towering gum-trees stand white and shining as if decked with silver, and almost without casting any shadow. Every line, every leaf, shows distinctly, still the penetrating clearness of day is wanting, and a mysterious veil lies over the shimmering landscape. Were I a poet I would try to depict the impression of this sight in the human mind. Being a naturalist, I feel the desire to analyse, and thus I ask myself: What is the reason of this wonderful effect, this magic and mysterious element in the moonlight landscape, which makes it an object of folk-lore and poet's fancy? The answer to this question is near at hand. The effect of moonshine owes its peculiar character to the circumstance of its giving light enough on clear nights to illuminate a surface directly exposed to its rays. Thus, for instance, an open book will be lighted up clearly enough to allow of your reading it. On the other hand, however, it is not powerful enough to call forth a perceptible brightness when diffused, *i.e. indirectly reflected* by the sky or the ground. To this diffuse light we owe, however, the possibility of recognising in daytime those parts of objects not directly exposed to the sun or even those that are situated in the deepest shade. The blackness of the shadows in the height of daytime is only a seeming one, brought about by contrast. . . . The contrary is the case with moonlight. Here the diffuse reflected light is so weak as to leave those parts of an object, which are not directly exposed to the rays, in darkness, and make their details disappear to our sight. The chief characteristic of moonlight thus consists less in the difference of its hue from that of the sun—though this, too, strikes us in a peculiar way—than in the absence of diffuse light and the almost absolute blackness of the shadows."

All through the book one finds facts made the basis of reflections; and this undoubtedly adds much to the pleasure of the reader. But even when Semon does not digress to philosophise about his facts, he manages to add to their interest by his enthusiasm. No one will read unsympathetically his account of his success with *Ceratodus*. For long he searched in vain for the eggs of this lung-fish; but one day Frank—the great good-for-nothing!—came running to the camp, "breathless and beaming with joy," thinking he had earned the reward of £1 promised to him who should first find the spawn. But Frank had not got the right thing, and it was about six weeks later that the "glorious discovery" was made. Semon tells of his troubles with the blacks, of difficulties in pickling the eggs and rearing the young fish: we mourn with him over some that were done to death by the excessive kindness of a girl-friend.

The later chapters of the book, relating to regions in which Semon stayed only a short time, are less satisfying than the earlier ones; but on the whole the work is to be recommended as full of interesting facts that are well told.

GREGG WILSON.

## THE LIVES OF STREAMS

River Development, as illustrated by the Rivers of North America. By Professor I. C. RUSSELL. 8vo, pp. xvi. + 327, with 23 figures and maps. London: John Murray, 1898. Price 6s.

This is a very useful work on an interesting subject; yet there remains the feeling that it might have been better. A little more systematic arrangement might have been accomplished; there would have been avoided some repetition, and such a divorce as to separate 'the migration of divides' from 'stream conquest.'

River development is so complex a subject, requiring to be considered under so many headings, and the various phenomena so interact among themselves that the difficulty of a concise arrangement may be granted. Nevertheless that is just why it is important. Such a work seems to require division into, for instance, (1) river mechanics, (2) river growth, individually, (3) river life-history, throughout a geographical cycle, and (4) growth and decline of what may be called river groups.

Now, while the author has admirably considered the first of these divisions, he seems not to have made sufficiently clear distinctions in regard to the others. To indicate what is implied it seems necessary to turn to biological language. Thus our second suggested division may be called river ontogeny—the growth from source to sea; the third, river phylogeny—the phases of development during a geographical cycle; the fourth, geographical distribution—territorial expansion or contraction. For a river's capture of territory is a phenomenon capable of separate consideration from that of river phylogeny, though of course it has an important bearing thereon.

The author often speaks of the lives of streams. He implies thereby the life throughout the geographical cycle. He speaks of young streams to imply those existing at an early date of the cycle. But this seems to lead to a certain want of precision, because one is apt to think of young streams as those near the source—the head-waters—irrespective of the phase of development of the geographical cycle. And so with other ages. A little reflection will show the importance of making distinction between the growth of a stream itself, on its way to the sea, and the sequence of phases which it exhibits as the geographical cycle runs its course.

These criticisms are not intended as fault-finding. They are to point out how much may be gained by keeping the phases of river development distinct.

One fault, however, does run through this work. The author introduces a term "corrasion" (p. 29) to denote "the deepening and widening of a stream channel." Etymologically it is most unfortunate. For *corrado* means to scrape or rake together. It might suitably be applied to the process whereby a stream makes a gravel-bed; it cannot denote the processes by which a stream carries material away.

The author is not responsible for the terms composed of combinations of "sequent" which he uses. Yet these are unfortunate; because they are apt to clash with ordinary non-technical words, so that it is difficult to avoid saying, not of course in a sentence, but on a page, that an obsequent stream arises subsequent to and consequent on the growth of a subsequent, which is a subsequent event to a consequent. It reminds me of a child's question—"Is a donkey called a donkey because it is a donkey?"—a delightful confusion of concrete, abstract, and qualitative senses of a word.

Speaking of subsequent streams, the author does not fully discuss their development (p. 185), partly for the reason that he only takes an outcrop plain into consideration. In regard to obsequent streams he is certainly at fault. He says, in effect, that when a stream is beheaded a portion of the down side stream is reversed to make an obsequent. But the slope of the channel would not permit this. What does happen is the result of a slow process, of the growth of a new drainage, whereby an altogether new stream is started.

But though exception may be taken in such matters, the book may be thoroughly commended as one which puts before the reader a most interesting amount of detail concerning river development. Particularly may it win approbation for some charming photographs. Unfortunately the same cannot be said for the maps; they are not at all clear. S.

## PAGE AND LAPWORTH.

Intermediate Text-Book of Geology. By CHARLES LAPWORTH, F.R.S., Professor of Geology, Mason University College, Birmingham. Founded on the Introductory Text-Book of Geology by the late DAVID PAGE, LL.D. 8vo, pp. xvi. + 414, with 174 figures. Edinburgh and London: William Blackwood and Sons, 1899. Price 5s.

Since the publication of the first edition of Page's "Introductory Text-book" in 1854, the twelve editions through which it has run have faithfully recorded the evolution of modern geology. The present excellent compendium of what is known on the subject may be viewed as the thirteenth edition, though so much expanded as justly to deserve Prof. Lapworth's new title. Part I. treating of Dynamical Geology, though brief, gives a fair summary of the external and internal agencies at work modifying the surface of the globe, special attention being directed to slow secular movements of the crust. In Part II. Petrological Geology, following upon a short chapter on aqueous rocks, a clear account is given of the igneous rocks, including a convenient working classification, while the chapters on the metamorphic rocks are particularly good, especially the résumé of the recent opinions on the various modes of formation of foliated rocks. In Part III. Historical Geology, after a general classification of the stratified rocks and a short chapter on palaeontology giving a modern classification of such animals and plants as are likely to occur in a fossil state, follows an account of each of the great formations as developed in the British Islands—their foreign equivalents being also indicated—and good figures of their characteristic fossils are given. As might be expected from Prof. Lapworth's experience among the older Palaeozoic rocks, the chapters on these formations are particularly strong, and the figures of the zonal forms of Cambrian trilobites and more especially of the graptolites of his Ordovician and Silurian systems form an especially valuable and new feature of the work. Another valuable new feature is the introduction of a series of geological maps of the British Isles, by means of which the student is made to realise how our country has been gradually built up around the axis of ancient crystalline rocks, the remains of which are now exposed in the North-West Highlands of Scotland and Ireland and a few small scattered inliers which peer through the newer formations of England and Wales.

The résumé given at the end of each chapter cannot fail to be of service to teachers. Altogether the book is excellent, and thoroughly recommends itself.

B. N. P.

## STILL THEY COME.

A Text-Book of Botany. By J. M. LOWSON, M.A. 8vo, pp. viii. + 394, with 247 figures. London: W. B. Clive and Co., 1898. Price 6s. 6d.

This is a preparatory book for London University Intermediate and Preliminary Scientific Examinations. It is well illustrated, but it has not acknowledged diagrams taken from other sources. The subject-matter, as far as space admits, is up to date, but too much condensed for a beginner to follow intelligently. The opening chapters are presented as summaries of facts before the student has become acquainted with individual plants, hence they are of little educational value. Chapter IX., on the Structure of the Flower, or Chapter VII., on Nutrition and Growth, slightly modified, would have made a better beginning. In the hands of a private student the book may hinder rather than help; under the guidance of a judicious teacher the pupil may use the book with advantage.

R. T.

# OBITUARIES.

## WILLIAM RUTHERFORD, M.D., F.R.S.

Born at Ancrum Craig, Roxburghshire, April 20, 1839;
Died at Edinburgh, February 21, 1899.

This eminent physiologist was a graduate of Edinburgh University (1863). After a period of continental study he became assistant to Prof. Hughes Bennett (1865), and Professor of Physiology in King's College, London (1869). He occupied this Chair for five years, during the last three of which he was also Fullerian Professor of Physiology in the Royal Institution. From London he returned to Edinburgh in 1874, as successor to Prof. Hughes Bennett. His great gifts as a teacher and as an organiser raised the physiological department in Edinburgh to a position of proud prominence, and many now famous workers served their apprenticeship in his laboratories. His most important researches were concerned with the action of drugs on the secretion of bile, and with the structure of striped muscle; but his work as a professor was even greater. A lucid but dogmatic teacher, an eloquent lecturer, a skilful experimenter, a laborious student, a lover of music,—a remarkable personality, he will not soon be forgotten.

## SIR JOHN STRUTHERS, M.D., LL.D.

Born at Dunfermline, 1823: Died at Edinburgh, February 24, 1899.

From 1841, when he began to study medicine in Edinburgh, till shortly before his death, John Struthers maintained an untiring industry. As Extra-mural Lecturer on Anatomy in Edinburgh (1847), as Professor of Anatomy in Aberdeen (1863-1889), as a member of the General Medical Council, as President of the Royal College of Surgeons, Edinburgh (1895-1897), as a student of the structure of Cetacea, he did what his hand found to do with all his might. He will be long remembered as one who helped to raise the standard of anatomical teaching, as a loyal servant of his University who contributed not a little to its advancement and extension, as a shrewd reformer of medical education, as an early exponent of evolution views in days when they were less popular than now, and as an anatomist with an infinite capacity for taking pains.

## WILHELM BARNIM DAMES.

Born June 4, 1843; Died December 22, 1898.

By the death of Prof. Dames Germany has lost one of its foremost palaeontologists. During the whole of his career, from 1871 onwards, he was associated with the University of Berlin, first as Custos in the Museum (1875), next as

Professor Extraordinarius (1878), and finally, on the death of Beyrich, as Professor Ordinarius (1891). He travelled extensively in Europe, and spent his vacations as far as possible in the study of stratigraphical geology. His University thesis (defended at Breslau, where he studied under Ferdinand Roemer) referred to the Devonian rocks of Silesia, and was published in 1868. Until 1881 most of his writings dealt with invertebrate fossils, and after that date he paid special attention to the Vertebrata. His best-known memoir is that on the Berlin *Archaeopteryx*, published in 1884. He also wrote important memoirs on *Plesiosaurus*, Tertiary Chelonians, Triassic fishes, and the extinct Cetacean *Zeuglodon*.

On January 18, Dr. KARL FRIEDRICH WILHELM CLAUS, Emeritus Professor of Zoology in the University of Vienna, well known for his text-books of zoology, and for his numerous researches on invertebrates. He was born at Kassel on January 2, 1835, studied at Göttingen and Giessen, and was professor at Göttingen and at Marburg before he went to Vienna.

CHARLES DRURY EDWARD FORTNUM, the mineralogist and antiquary, died on March 6, at Stanmore, in his eightieth year. Dr. Fortnum divided his collections of Australian insects between the British Museum and the Hope Collection at Oxford many years ago. In his later years he paid especial attention to majolica and gems, and was a liberal donor to the University of Oxford. He was a trustee of the British Museum.

On February 9, at the age of 81, Prof. KARL MÜLLER, till recently editor of *Die Natur*, of which he was one of the founders. The Rev. J. DIGGES LA TOUCHE, the geologist, at Stokesay in February of advanced age. He was the author of "A Handbook of the Geology of Shropshire," 1884; was President of the Caradoc Field Club; and made a geological model of the country around Stokesay. Prof. H. F. WÜSTENFELD, of Göttingen, who died at Hanover towards the end of February, at the age of 91, was well known for his "Geschichte der Arabischen Aerzte und Naturforscher." Dr. FRANCIS N. MACNAMARA, on March 5, at the age of 67, a prominent member of the Indian Medical Service, once Professor of Chemistry at the Calcutta Medical College. On March 13, from the plague, Major J. EVANS, Professor of Pathology in the Calcutta Medical College.

The death is also announced of the famous palaeontologist, Prof. CHARLES OTHNIEL MARSH of Yale. He was born in 1831, graduated in 1860, and became the first professor of palaeontology at Yale in 1866. An obituary notice will be published in our next number.

# NEWS.

The following appointments have recently been made :—Dr. R. T. Anderson, as instructor in histology and embryology in Harvard Medical School; Dr. Erich von Drygalski, as professor of geography at Tübingen; Dr. H. Held, as professor extraordinarius of anatomy at Leipzig; Dr. A. Y. Grevillius, to be assistant in botany in the Agricultural Experiment Station at Kempen on Rhine; A. W. Hill, as demonstrator of botany in Cambridge; Wladimir J. Palladin, as professor of botany in the newly established technical school in Warsaw; Prof. Fritz Regel of Jena, as professor of geography at Würzburg; Prof. W. F. R. Weldon, as Linacre professor of comparative anatomy at Oxford, in succession to Prof. E. Ray Lankester.

The resignations are announced of Dr. G. K. Niemann, professor of geography and ethnology in the Indian Institute at Delft, Holland, and of F. W. C. Areschoug, professor of botany in the University of Lund, Sweden.

Prof. Sir William Turner has been elected president of the British Association for the Bradford meeting.

Prof. E. Ray Lankester has been elected a correspondent of the Paris Academy of Sciences, and an associate of the Belgian Academy of Sciences.

The degree of LL.D. has been conferred by Aberdeen University on Mr. Chas. Stewart, F.R.S., curator of the Museum of Royal College of Surgeons, England; and on Mr. Geo. F. Stout, lately lecturer on comparative psychology at Aberdeen.

The degree of M.A. *honoris causa* has been conferred by the University of Cambridge on Dr. G. Sims Woodhead, the new professor of pathology.

Dr. Helmert, professor of geodesy in Berlin, has been elected a corresponding member of the Paris Academy of Sciences in the section of geography and navigation.

The statue of Helmholtz by Herter is completed, and will be placed in front of the University of Berlin, between those of the Humboldts.

The late Prof. W. Rutherford has left to Edinburgh University his valuable medical library and his collections of microscopical specimens and diagrams.

The annual statistical report of Glasgow University for 1898 shows a total of 1836 students, of whom 264 were women. The number in the faculty of Science was 117, as against 673 for Medicine, 762 for Arts.

The *Scientific American* records that the Indian Government has been offered by Mr. Jamestsji Tata the sum of a million and a quarter dollars for the establishment of a university for research on the model of Johns Hopkins.

A site has been secured at Kemp Town, overlooking Queen's Park, Brighton, for the Gardens of the recently started Zoological Society of Brighton and Hove. The managing directors are the Earl of Llandaff and Mr. F. W. Frohawk.

On April 9, a monument to Pasteur will be unveiled at Lille, and a Pasteur Institute opened.

The *American Naturalist* reports that the University of Indiana will have its biological station this year at Warsaw, Ind., that a permanent station in connection with Tuft's College is proposed on the rich shores of Casco Bay, U.S.A., and that the Natural History Society of St. Petersburg has established a biological station on the shores of Lake Bologoy.

A magnificent laboratory for the application of the Röntgen rays, under the directorship of Dr. Mezquita, has been recently opened in Madrid.

According to the *American Naturalist* the department of scientific investigation of the United States Fish Commission has awakened to new life under the supervision of Prof. Bumpus, and it aims to aid science in every way not incompatible with the main business of fisheries. The laboratory at Woods Holl is to be kept open throughout the year, and students will be welcomed there at any time.

In presenting to Prof. Lapworth the Wollaston Medal, the President of the Geological Society of London, Mr. Whitaker, made use of the following words :— "I am glad to know that in an elaborate memoir on the Silurian Rocks of Scotland, shortly to be published, the officers of the Geological Survey do full justice to your series of papers on these rocks in the South of Scotland, saying that you have furnished a complete solution of the stratigraphical and palaeontological difficulties of the subject, and have discovered the key to the complicated stratigraphy of the region. The summary, at the end of the chapter on Previous Researches, is written to place clearly on record the distinctive features of Prof. Lapworth's achievements, . . . which resulted in establishing the true order of succession of the strata." These are words alike honourable to the Geological Survey and just to the leader of geological thought and teaching of our time.

At a meeting of the Royal Society on March 9, Mr. A. C. Seward read a paper on the structure and affinities of *Matonia*, an isolated type among existing ferns, on the whole approximating more closely to the Cyatheaceae than to any other family. The data furnished by an examination of the palaeontological evidence led to the conclusion that in *Matonia* we have a survival of a family, now represented by two species in a few localities in Borneo and the Malay Peninsula, but widely distributed in the Mesozoic epoch, and especially abundant in the European area.

At a meeting of the Royal Society of Edinburgh on March 20, communications were read by Sir John Murray on the temperature over the floor of the ocean and the maximum and minimum temperature of the surface ; and by Prof. J. Cossar Ewart on intercrossing and variation.

At a meeting of the Geological Society of London, on March 8, Dr. Arthur Rowe read a paper on the genus *Micraster*. The author showed that definite features in the test were confined to definite zones in the white chalk, and thus supplied a valuable key to the determination of age of this deposit, in cliff or inland sections. Dr. Rowe promised a further paper on the zones in the coast sections of the white chalk, based on a study of these echinoderms, a paper which should prove of considerable interest and value to professional and unprofessional geologists.

As retiring president of the Biological Society of Washington, Mr. L. O. Howard gave an address on the economic status of insects, which is printed in *Science* (1899, ix. pp. 233-247). The *pros* are six and the *cons* five, as stated in this address which is full of entertainment, *e.g.* the reference to the prophet Joel as "an agricultural pessimist," or the discussion of Isaac Weld's note that General Washington told him that the mosquitoes of Skenesborough "used to bite through the thickest boot."

At the meeting of the Hull Scientific and Field Naturalists' Club on Wednesday, March 8, Mr. J. A. Ridgway of Beverley gave a lecture on "Shooting Stars," describing *inter alia* the famous shower of November 1866, which he witnessed. At a sectional meeting held the previous Wednesday evening, Mr. D. Murray of Kilnsea, near Spurn, sent for exhibition the skull of an elephant which had been obtained from the beach—a curious fragment of jetsam, doubtless admitting of a prosaic enough explanation.

Mr. G. Abbott, honorary general secretary of the S.E. Union of Scientific Societies, asks us to notify that the date of the next congress, which will be held at Rochester, has been altered to May 25–27.

On February 18, Prof. L. C. Miall lectured at the Royal Institution on "A Yorkshire Moor," and his lecture—a charming study in natural history—is now printed.

At the Congrès des Sociétés Savantes to be held at Toulouse, April 4 to 8, there will be an exhibition of timekeepers, instruments, etc., divided according to the decimal system. The Geographical Society of Toulouse invites those interested in this reform to send instruments, diagrams, and published papers.

Among the prizes offered by the Reale Istituto Lombardo are the Fossati prizes for 1899 on the macroscopic or microscopic structure of the nervous system, for 1900 on the regeneration of peripheral nerve-fibres in vertebrates, for 1901 on the structure of the brain in higher animals.

A prize of 500 francs is offered by the Société de physique et d'histoire naturelle de Genève for the best monograph on a genus or family of plants. The memoirs, which may be in English, must be sent to Mr. Pictet, the president of the society, before January 15, 1900.

The Belgian Royal Academy has awarded prizes to Georges Clautriau, of Brussels, for his work on digestion in carnivorous plants, and to L. Cuénot, of Nancy, for his work on the excretory organs of molluscs.

At a meeting of the Edinburgh Geological Society, on March 16, communications were read on "Contact metamorphism round the granite of the Cheviots," by Mr. Herbert Kynaston; "The granite intrusions and allied rocks in the neighbourhood of Braemar," by Mr. E. H. Cunningham-Craig. Dr. Hepburn and Mr. Simpson described antlers of red deer and other remains from the superficial deposits of Hailes.

The Joint Committee of the Royal Society and the Royal Geographical Society, appointed to promote a National Antarctic Expedition, made application some time ago to the Council of the Royal Society and the Council of the British Association for grants of money in aid of the proposed expedition. The treasurer of the Royal Society has applied on behalf of the Council to the Government Grant Committee for a grant of £1000, and the Council of the British Association will recommend to the next meeting of the general committee that a like sum be contributed by the Association. There is every reason to believe that these grants will be made.

The president of the Royal Geographical Society, Sir Clements Markham, has received a letter from Baron von Richthofen, president of the Berlin Geographical Society, in which he refers to a meeting recently held in Berlin in connection with the German Antarctic Expedition. Addresses were delivered by Baron von Richthofen, Dr. Drygalski, who will be leader of the expedition, Count D'Arenberg, president of the Colonial Society, and others, and the greatest enthusiasm was displayed by the audience of some 1500 people. As did the speakers at the meeting, so Baron von Richthofen in his letter refers in very explicit terms to the expected co-operation of England in the great work of a thorough exploration of the unknown Antarctic area. The German Committee have laid down the different routes which the two expeditions might follow, in order that the joint work may be as effective as possible, while each expedition would be entirely independent of the other so far as internal organization and command are concerned. "There cannot be any doubt," says the *Times*, "that if the two expeditions, adequately equipped, carried out their work on a common plan, the gains to science would be immense, much greater than if the expeditions carried on their work entirely independently of each other. Moreover, from other points of view, it is desirable that two great and friendly nations like Germany and England should co-operate in promoting common interests whenever it is possible. It would therefore be in the highest degree humiliating if

Baron von Richthofen had to be informed that England cannot co-operate with Germany in this great work because the British Government decline to support an expedition, and wealthy Englishmen are not public-spirited enough to subscribe the few thousands necessary."

We are glad to learn that the Millport Marine Station, one of the youngest of British Marine Biological Stations, bids fair to attain the success which has fallen to some of its older rivals. During the past year the station has been in full working order, and the fact that the tables in the laboratory were occupied by working naturalists for varying periods on thirty-eight different occasions, shows that the station is both needed and appreciated. The committee must also feel much gratified at the interest shown in the Robertson Museum, which was visited by over 8000 persons last year. Many additions have been made to the station during the past year, but the Committee appeal for more funds to enable them to effect further improvements, and also for donations to the library of books or papers on marine zoology.

Prof. Carl Chun writes to Sir John Murray (*Scot. Geogr. Magazine*, 1899, xv. p. 143) reporting on the course of the German Deep-Sea Expedition after leaving Cape Town. They re-discovered Bouvet Island so often searched for in vain—apparently a volcanic cone entirely covered with glacier ice. Especially remarkable is the series of soundings taken on the edge of the pack from Bouvet Island to the neighbourhood of Enderby Land. A depth of over 3000 fathoms was frequently sounded, showing that the Antarctic Sea is much deeper than has hitherto been supposed.

The Socotra Scientific Expedition, led by W. R. Ogilvie Grant and H. O. Forbes, has returned to London after a very successful exploration of the island. The collections made include several remarkable forms, especially some beautiful butterflies. A wild donkey, said to be a species new to science, has been secured for the Zoological Gardens. A great part of the interior of Socotra has been surveyed, and correct geographical data obtained for a revised map.

The expedition despatched by the Imperial Academy of Sciences of Vienna arrived at Socotra shortly before the departure of the British party.

The Russian Geographical Society, with the aid of funds supplied by the Tsar, has organised a new expedition for the exploration of Central Asia. It will leave St. Petersburg this month under command of Lieutenant Koslow, and will probably occupy two years.

Mr. John S. Budgett, sent out to the Gambia by the Zoological Society of London, is meeting with success in his collecting, among his captures being large specimens of *Polypterus*.

The *Scientific American* announces that S. H. Cavendish, explorer, and Edward Dodson, the naturalist, "have gone to Patagonia to search for the giant ground-sloth, or megatherium."

A grant of £300 from the Works Travelling Scholars Fund, Cambridge, has been made to Mr. Skeat in aid of his scientific expedition to the Malay Peninsula.

Dr. K. Kishinouye, of the Imperial Japanese Fishery Bureau, arrived at Tokio on March 15. Since leaving England in August 1898, his work has taken him to France, Germany, Norway, Sweden, Finland, Russia, and Italy.

In a Chicago school, in which at least a thousand children are taught, experiments will be made with an "ergograph" for measuring mental endurance, the object being to determine the "fatigue period" when school work should not be pressed. We are not informed as to the nature of the ergograph, but it is said to be "adapted for the purpose."

The Association of American Anatomists has accepted the proposal of the editors that the *Journal of Anatomy and Physiology* should be the official organ of the Association, and Prof. G. S. Huntington, of Columbia University, has been nominated American editor.

# Natural Science

A Monthly Review of Scientific Progress

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## NOTES AND COMMENTS.

### Moulting as a Means of Defence.

It was Romanes who said, "Wherever we tap organic nature it seems to flow with purpose," and post-Darwinian naturalists have far excelled the authors of the Bridgewater Treatises in the ingenuity of their search for the utilitarian justification of occurrences. It must be admitted, however, that there are some vital processes where the seamy side is conspicuous, and one of these is ecdysis or moulting. The expensiveness, the fatigue, the riskfulness of the process cannot be ignored, even if the progress of research should make its physiological inevitableness much more intelligible than it is at present. It is with interest, therefore, that we note a communication by Künckel d'Herculais (*Comptes Rendus Acad. Sci.*, Paris, cxxviii. 1899, pp. 620-622) in which it is pointed out that the moulting of insects is a means of defence against animal and plant parasites. In seeking to infect young locusts (*Schistocerca peregrina* Olivier) with the spores of the fungus (*Lachnidium aeridiorum* Giard) which he discovered on the adults, Künckel d'Herculais was struck by the fact that the repeated moultings—every eight days—made the fixation of the spores on the integument exceedingly difficult. Even if entrance be found through the stigmata, the casting of the internal lining of the tracheae is effectively antagonistic to infection. In the same way, the casting of the intestinal lining was shown to free another species of locust (*Schistocerca paranensis* Burmeister) from a burden of Gregarine parasites. Thus there is advantage as well as disadvantage attendant on the physiologically inevitable process of moulting, and there is some practical interest also in what the author has observed, since the moulting will evidently tend to hinder the elimination of the locusts by inoculation,—a method which has been of late regarded with some hopefulness in various locust-infested countries.

### Nitragin and Nodules.

One of the last communications on the much-discussed problem of the nodules of leguminous plants is a paper by Miss Maria Dawson

(*Proc. Roy. Soc.* lxiv. 1899, pp. 167, 168). The author has been led to an unhesitating confirmation of the parasitic nature of both the filaments and the bacteroids contained in the nodules. The filaments, it was found, have no constant relation to the nucleus of the cells as was represented by Beyerinck in 1888. The infection-tube which grows into the root-hair consists of strands of straight rodlets, which burst out here and there, and become transformed into X, V, and Y-shaped bacteroids, incapable of further growth. As the rodlets multiply by fission, the author refers them to the Schizomycetes, but leaves it an open question whether they are true bacteria. Commercial nitragin *does* consist of the tubercle organism, as its advocates assert, and as a result of the inoculation of either seeds or soil with it, tubercle formation ensues. In the commercial product the organisms are present as micrococcus-like bodies, all straight and immobile, which become converted into bacteroids and straight rods in pea-extract. The cautious practical conclusion is that the addition of nitragin to soils rich in nitrates appears to be inadvisable, but a supply of it to soil poor in nitrates results in an increased yield, though better results are obtained if, instead of nitragin, nitrates be added to the soil.

## Classification of Variations.

ABOUT a year ago, Professor G. Schwalbe delivered before the German Anatomical Society an interesting address on the classification of variations, which seems to have received but little attention in this country. It is to be found in the *Ergänzungsheft* to the fourteenth volume of the *Anatomischer Anzeiger*, and well deserves the consideration of evolutionists. With some of his categories of classification all who have interested themselves in the subject of variation are familiar, the distinctions between continuous and discontinuous, substantive and meristic variations, and between blastogenic or germinal *variations* and somatogenic *modifications;* but there is at the end of the address a contrast which we do not remember to have seen so clearly put before. It is that between those variations which may be described as due to incompleteness in the inheritance, and those which cannot be so described, being distinctly "new departures," possibly due to the influence exerted on the germ-plasm by environmental stimuli, either directly or by changes evoked in the metabolism of the body.

## Local Colour in Universities.

WE referred some months ago to Mr. Chamberlain's wise remark at Birmingham that universities should have local colour, that they should

in some feature of their organisation be eminently adapted to their social milieu. A predominantly classical university at Birmingham might commend itself to those who believe in taking a cold to cure a fever, but there is little reason to believe that it would be practically effective. It would be like making a special feature of mining at Aberdeen, or of polar exploration at Khartoum, or of commercial education at St. Andrews, or of marine biology at Moscow. With these sentiments it was with much pleasure that we read the first paragraph of the Rectorial address delivered on the 6th of March by Professor Karl Brandt at Kiel.

That the University of Kiel, he said, is a university by the sea, has had as a natural result that many members of the professoriate have turned their attention to the problems which the sea always presents. Especially has this been the case since 1870, when the Kiel Commission was charged with the scientific investigation of German seas, to the end that their practical exploitation might be more effective. "Not only for zoologists, botanists, and oceanographers, but for chemists, physicists, physiologists, histologists, and hygienists, the sea which lies before us presents valuable material for investigation. And the fact that a number of the University teachers are also connected with the Marine Academy has brought representatives of various disciplines, which might otherwise have had little real touch with salt water, face to face with the manifold relations between man and the sea. Our University, which formerly had rather the character of a 'Schleswig-holsteinische Landsuniversität,' has been gradually becoming the marine university of Germany." And it is well known that the first great scientific marine expedition of Germany, the "Planton Expedition," was manned from Kiel. Professor Brandt's words seem to us exceedingly wise, and while we rejoice that adequate recognition is given to the resources of the sea in St. Andrews and Liverpool, to navigation in Glasgow, to bacteriology in London, to geology and good-breeding in Edinburgh, and so on surely all round, we still think that the question may be profitably asked, whether our universities have as yet sufficiently realised the importance of acquiring that local colour which is so strongly justified on scientific and artistic grounds, in short, by common sense.

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## The Metabolism of the Sea.

Professor Karl Brandt speaks hopefully of the progress of research in regard to what may be called the metabolism of the sea. The long-continued observations by chemists and biologists have given us some sort of picture of the circulation of matter in "agriculture," and the same is being attempted for the much vaster areas of "aquiculture." The careful studies made by Susta on the carp of the fish-pond and

the ambitious plankton statistics of Hensen and his colleagues are two widely separate instances of the many-sided attack which is being made on the problems of "Stoffwechsel" in aquatic life.

In his address, Professor Brandt has naturally much to say concerning nitrogen, and, at the risk of being trite, we may summarise his general argument, so that the point of his final suggestion may be made clear. The sources of nitrogen available for plant-life are nitrates, nitrites, and ammonia, to which must be added the nitrogen of the atmosphere in so far as that may be brought into available form by electric discharges and by help of the symbiotic fungi in the root-tubercles of Leguminosae. From these simple raw materials the plants elaborate the albuminoids which form the fundamental nitrogenous supplies of animal life. On the death of the animals part of the nitrogenous material becomes by putrefaction again available as food for plants. Bacteria affect the circulation in two ways:—on the one hand, over the whole earth there are nitrifying bacteria by help of which ammonia becomes the origin of nitrites and nitrates; on the other hand, there are denitrifying bacteria which effect the reduction of nitrates and nitrites, so that ammonia results, and also some free nitrogen which passes out of the circle of life. So it is in dry land, and so in the sea. Plants raise inorganic substances to an organic level; animals feed upon and return food to plants; and bacteria affect the circulation of matter in two directly opposite ways. The land is always losing nitrogenous substances to the sea, and the only known ways in which the land is compensated for its loss are by electric discharges in the atmosphere and by the root-tubercles. This consideration suggests at first sight a possible poisoning of the sea with nitrogenous substances in the course of millions of years; but the suggestion is probably fallacious, since it ignores the action of the denitrifying marine bacteria which destroy any superfluity of nitrogenous compounds in the water, and thus tend to preserve the balance of nature.

After expounding Hensen's plankton-method, to which he attaches great importance as affording a sure statistical basis for physiological conclusions, Brandt discusses two results which are of much interest. The first is, that on the whole the shallow seas are richer in plankton than the deep areas, of which the Sargasso sea, for instance, may be cited as a very sparsely peopled area. The explanation of this is to be found in Liebig's law, that there must be a minimum of each of the eleven or twelve essential chemical elements present if plants are to thrive. In the shallow seas this minimum is secure, for the land is nearer and the area of distribution less vast; in the deep seas the material is so much more widely distributed that the nutritive medium becomes, so to speak, too thin. Apstein has shown that the quantity of pelagic life in lakes varies in direct proportion to the quantity of nitrates present. The second conclusion—perhaps the most striking which has yet resulted from plankton studies—is that the tropical and

sub-tropical seas are relatively poor in plankton, while the arctic seas are relatively rich. How different the terrestrial conditions are, we all know. That plants need light and warmth is also a familiar fact, and yet if Hensen's results are trustworthy we have to face the conclusion that the richest plankton is in the dark, cold arctic seas. Brandt finds a provisional solution of the riddle in the theory that the low temperature hinders the action of the denitrifying bacteria, and that the colder waters are therefore richer than those in the tropics, where the waste of the precious nitrogenous substances is wanton.

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## The Organism and the State.

Biology, according to Professor Oscar Hertwig, is pre-eminently the science of the organism, "die Lehre vom Organismus," and as such it has some light to throw on "Sozialwissenschaft," which has the social organism as its central problem. That this familiar thesis should form the subject of a "Festrede" in the University of Berlin may be of value in convincing the narrow-minded that the boundaries between the various sciences are mainly artificial adaptations to intellectual convenience, and that no science can rise to its full dignity in isolation. The address ("Die Lehre vom Organismus," etc. Gustav Fischer, Jena, 1899, pp. 36) is characterised by Hertwig's usual clearness, and its leading thought is this—the problem of the organism has been attacked by three methods, each essential and each partial; there is the method of chemical analysis, the method of physical analysis, and the method of "anatomical-biological" analysis; the first shows us the organism as a "tourbillon" of chemical molecules, the second reveals a series of transformations of energy, but the third method is necessary if we are not to lose the gist of the whole matter that the organism is a unified organisation capable of effective response to the order of nature. Devotion to one particular line of analysis has in the history of biology often led to partial truths which have been errors; it behoves us to seek to free ourselves from such partiality, and not for the sake of biology only, but because of its relation to social science.

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## A Tail in place of a Head.

There is a certain fascination in the subject which Professor Weismann discussed in our last number. The phenomena of regeneration are often surprising and sometimes quaint, and the interpretation of them as adaptive gives pleasing scope for ingenuity. Whether this interpretation, which Weismann has so powerfully defended, be correct or not, time will show; meanwhile each fresh fact is a gain, and one of

the last put on record is that of an earthworm regenerating a tail in place of a head. We are wrong, however, in calling this a fresh fact, for it was observed by Spallanzani in 1768, and is now confirmed by Professor T. H. Morgan (*Anat. Anzeig.* 1899, xv. pp. 407-410, 9 figs.). We have often admired the "expeditiousness" of American workers, but when Mr. Morgan tells us that he began in October 1899 a new series of experiments, we cannot help regarding it as a little "previous."

In the cases observed many segments were regenerated from the anterior end of a posterior half of a worm, and the new part was a tail, not a head. Sections showed that there was no brain, and that the ventral cord extended into the last (youngest) segments of the new part. Moreover, to the objection that such parts might subsequently develop a brain at the distal end, Mr. Morgan answers that the nephrostomes in the new part are turned backwards, that is towards the old part, and the main part of the nephridial tube lies in the segment distal to the one containing the nephrostome. Therefore what is produced is a worm with two tails turned in opposite directions. It is, if that makes any one any wiser, a case of heteromorphosis.

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## Regeneration in Starfishes.

MANY naturalists have made observations on the regenerative power of starfishes, but Miss Helen Dean King has advanced our knowledge of the subject by precise experiments (*Arch. Entwickmech.* 1898, vii. pp. 351-363, 1 pl.). As Professor Weismann's recent essay has focussed the subject, and as it is important to test his theory by reference to all known cases, it is appropriate here to summarise Miss King's chief conclusions in regard to *Asterias vulgaris*. New arms develop only from the disc; an isolated arm must have at least a fifth of the disc if regeneration is to occur; the aboral surface of the disc may be regenerated; the ventral portion of an arm can regenerate the dorsal surface, but the reverse does not seem to occur. If a starfish is cut in two, each half may regenerate the whole, but there is no evidence that this occurs in natural conditions; moreover, two halves of different individuals made be made to unite. In general, we get the impression that the regenerative capacity in the starfish is well adapted to cover the ordinary contingencies of life and a good deal more.

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## An Alpine Paradise.

MR. H. J. ELWES, in an interesting paper on the zoology and botany of the Altai mountains (*Journ. Linn. Soc.* (Zool.) 1899, xxvii. pp. 23-46,

6 figs.), gives us a glimpse of a wonderful field for research, within three weeks' journey of England, and yet "practically less known to naturalists than many parts of Central Africa." Speaking of the marshy Alpine flower-gardens near his camp on the Darkoti or Tachety river, 30 miles south-west of Kuch Agatch, at about 7000 feet, he says, "I have never, either in the Alps of Europe, in the Sikkim Himalaya, in Colorado, California, or anywhere else, seen such a perfect natural garden of beautiful Alpine flowers as I saw here in the middle of July. Among the most conspicuous were the lovely *Primula nivalis* Pall., which strongly resembles *P. parryi* of Colorado; *Dracocephalum grandiflorum*, which grew in sheets of caerulean blue; *Polemonium pulchellum; Gentiana altaica; Pedicularis verticillata, P. foliosa, P. comosa; Allium sibiricum*, or *senescens*, the most ornamental of its genus; *Linum caeruleum; Iris tigridia* Bunge; *Pyrethrum pulchellum;* a lovely blue *Corydalis* growing in wet places, which Mr. Baker cannot name, and which may be new; a beautiful *Aquilegia*, named *A. glandulosa* at Kew, but much finer than that plant as we know it in our gardens; several pretty species of *Astragalus, Lloydia serotina;* and many well-known Arctic and high Alpine plants, such as *Papaver alpinum, Draba ochroleuca*, and *Saxifraga oppositifolia*, which were found as high up as 8500 feet, where the flora and scenery reminded one strongly of the high fjeld of Norway, and *Dryas octopetala*, which covered the curious dry gravelly ridges on the hill-sides in many places." Truly an Alpine paradise.

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## Regeneration and Development.

In connection with Professor Weismann's article on regeneration in the last number of *Natural Science*, it is of interest to call attention to a "Rectoratsrede" by Professor H. Strasser of Bern, entitled "Regeneration und Entwicklung" (Gustav Fischer, Jena, 1899, pp. 31, 1 mark). In this interesting address a comparison is made between the phenomena of regeneration and those of ordinary ontogeny, and an attempt is made to suggest a compromise between Weismann's position and Hertwig's. It is interesting to see how Strasser's criticism that Weismann attaches too little importance ("eine allzu untergeordnete Bedeutung") to the interactions of parts and the influence of external conditions has been met in Weismann's recent article, as on p. 322: "I may, indeed, have laid too little emphasis on the *rôle* of the liberating stimuli, and bestowed my attention too exclusively on the 'Anlagen' . . ." Or, again, "It will probably be necessary to make a compromise between the theory of dispersal and that of liberation, though it may not yet be possible even to sketch its outlines with certainty."

## The Bipolar Hypothesis.

PROFESSOR D'ARCY W. THOMPSON has subjected the so-called "bipolar hypothesis" to careful scrutiny and has found it wanting. In the introduction to his paper (*Proc. Roy. Soc. Edin.* 1898, pp. 311-349) he points out that Theel was one of the first to suggest that a peculiar likeness exists between the northern and southern extra-tropical faunas, and particularly between those of the arctic and antarctic regions. The suggestion was elaborated by Pfeffer, and has of late been dealt with in great detail, and in relation to the antecedent causes that might have led to such a phenomenon by Sir John Murray. Dr. A. E. Ortmann and Professor C. Chun have both pronounced against it, the latter believing that the occurrence of a small number of forms common to far northern and far southern seas may be sufficiently accounted for by the continuous distribution or gradual intermixture of forms in the depths of the intervening oceans under present conditions, without our needing to have recourse to an explanation of the phenomenon in the different conditions of a former age.

Professor Thompson's analysis "shows that of some ninety species quoted by Murray as common to northern and southern localities while absent from the intermediate zone, there are more than one-third in which grave doubt as to their identification was expressed by the original describers, or in which the identification has been doubted or denied by later writers.

"In somewhat more than another third the evidence of identity is inconclusive or even inadmissible by reason of the nature of the examination to which the specimens were subjected (as in the case of the horny and calcareous sponges), or by reason of the small size of the objects and lack of adequate marks of characterisation (as in the case of the minute ostracod and molluscan shells).

"Of the remaining forms about a dozen find their northern representatives in the Japanese seas, where they form part of a fauna predominantly southern in its relations, and where at least the occurrence of any particular form cannot be taken, *ipso facto*, as evidence of a boreal centre of distribution.

"Both these last forms and the remnant of equal number that are quoted as occurring in the North Atlantic, as well as in or near the Southern Ocean, are for the most part deep-water species, and have in a large proportion of cases peculiar characters of their own. We cannot say at present that they are forms characteristic of any particular geographical province, and their specific area of distribution has in some cases been greatly extended since the date of their original discovery."

We have quoted the above paragraphs since the problem is one of great interest and importance, and to show that the indictment of the hypothesis is a heavy one. The author proceeds to point out that the

bipolar hypothesis has been rejected by Professor Herdman for the Tunicates, by Professor Ludwig for the Holothurians, by Dr. Ortmann for the Crustaceans, and that his own analysis has not revealed a single species of fish, of Decapod, of Isopod, of Amphipod, which inhabits at once the arctic and antarctic oceans.

He admits that the "bipolar hypothesis" might still find support if it could be shown that similar and truly allied forms gave to the two regions a common facies. But of this he finds no convincing evidence.

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## Eocene Sharks and Skates.

THE Selachian fauna of the Eocene period is very imperfectly known. There are a few complete skeletons in the fissile limestone of Monte Bolca and Monte Postale in Northern Italy, and a nearly complete skeleton of *Xiphotrygon*, which was described by Cope, from the Green River marls of Wyoming. In other localities they are represented merely by portions of dentition, detached teeth, cartilage, or vertebrae. As, however, detached fragments are not always distinctive, and the generic or specific assignation of them is little more than guesswork, we are unable to restore the Eocene Selachian fauna from present materials; Mr. Smith Woodward, however, in the *Proceedings of the Geologists' Association* for February, has brought together the available evidence from British deposits, and placed it on record in the hope that further search may lead to better material and more accurate knowledge. A plate of fossil teeth accompanies the paper.

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## Radiolaria in Devonian Rocks.

THE discovery that Radiolaria were to be found in the older cherts has been taken up and worked for all it is worth by Dr. J. G. Hinde, who has again contributed a paper to the Geological Society. The present specimens come from the district of Tamworth, New South Wales, and are found in siliceous limestones and volcanic tuffs. Those in the tuffs are better for study than those in the limestones, because they occur more isolated. At present the hunting of fossil Radiolaria seems highly exciting, and comparable to that of insects, for every one found appears to be a new species, and provides for endless ingenuity in the coining of new names. But, as Dr. Hinde truly says, "our present knowledge of Palaeozoic Radiolaria is limited to those occurring in a few outcrops in Central and Northern Europe," and so rounds off his fifty-three new specific names.

## Foraminifera of Borneo.

An interesting and important paper on the orbitoid and nummuline Foraminifera of Borneo has been published by Messrs. R. B. Newton and Richard Holland in the *Annals and Magazine of Natural History* for March. The paper should be of especial value to those who are at present unravelling the age of the rocks of various coral islands, the bulk of which have to be studied as thin sections. The authors have shown a wise discretion in not instituting numerous new species, so easy from the deceptive nature of sectional material. The one form described as new is *Orbitoides* (*Lepidocyclina*) *verbeeki*, whose distinctness was only established after careful comparison. It is interesting to see another identification of Schlumberger's genus *Linderina*, first described from the Upper Eocene of Bruges (Gironde). The material dealt with was provided by the late Mr. A. H. Everett and by Professor Molengraaff, now of Pretoria. The type of *O. verbeeki* is founded on Brady's *O. papyracea* (1875), and the specimens are partly at Cambridge and partly in the British Museum.

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## On Ciliary Movement.

In the northern city in which the editorial and publishing offices are now placed, the inhabitants not infrequently complain of the excess of water which pours down from our too liberal skies, but it does not often happen that we are absolutely drowned out of house and home. This sad calamity has now overtaken us, and, dripping and disconsolate, we are left to survey the ruins of what was lately a snug sanctum. But the impatient reader may ask what the editorial woes have to do with ciliary movement. The answer necessitates a short explanation. Professor Herrera has recently sent us an apparatus to illustrate a paper ("Explication du mouvement vibratile," *Mem. Soc. Alzate*, x. p. 322) which he has just published on the causation of ciliary movement. In the apparatus a stream of water is employed as a substitute for the osmotic currents which the author believes to be the prime cause of the movements of living things, and the Professor urged us strongly to attempt the experiment for ourselves. The apparatus, it may be well to explain, consists of a large artificial cell furnished with numerous short rubber tubes representing cilia, and a large tube by means of which the stream of water may be introduced into the cell. The directions informed us that when the large tube was fixed to a tap, and the tap turned on, the small tubes would vibrate rapidly. Full of faith and enthusiasm we made the experiment, the office boy assisting with a devotion which his legitimate work often fails to inspire. We regret that we are unable to give a clear account of the

movements of the cilia, or to speak of the theoretical bearing of the results, but we can honestly certify that as a douche the apparatus is of the highest practical value. We have no wish to throw cold water upon Professor Herrera, but we would like to mention that he certainly performed the operation very successfully upon us. Indeed, when once again the editorial person was restored to comfort and dryness, a sudden suspicion flashed upon us that perhaps the instrument had been tampered with in transit, and that some enthusiast, too hardly treated in these pages, was now avenged, having successfully returned *quid pro quo*.

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## The Justification of Sea-Fish Hatching.

THE recent rapid development both in this country and America of the practice of artificial hatching of sea-fish eggs, has, naturally enough, raised up critics who question the economic value of the process. In the "Report for 1898 on the Lancashire Sea-Fisheries Laboratory," Prof. Herdman devotes several pages to a consideration of one of these criticisms. His paper is of much interest, especially when taken in conjunction with the account given elsewhere in the report of the work done at the hatchery at Piel. The critic, Mr. Fryer, in the "Twelfth Annual Report of the Sea-Fisheries Inspectors for England and Wales (1898)," discusses the methods and results of American hatcheries as disclosed by their annual reports, the discussion tending to disprove the usefulness of sea-fish hatching in general.

As to methods, special emphasis is laid on one point. It is well known that two plans have been adopted to obtain eggs for hatching purposes. The one, that hitherto practised at Piel, consists in obtaining spawning fish from trawlers, or floating eggs from the surface; the other, as practised in most of the older stations, consists in keeping mature fish in captivity in spawning ponds until they breed. Mr. Fryer argues strongly in favour of the former course, urging especially that there is every reason to believe that the captive fish breed more rarely and produce fewer ova than they would under natural conditions. In other words, he argues that however successful the hatching operations, the resulting fry are far less numerous than they would be under natural conditions. On the other hand, Prof. Herdman points out that the work at Piel has proved, if indeed it needed to be proved, that the difficulties in the way of collecting eggs from trawled fish are so great as to reduce the chance of successful hatching almost to a minimum—experience shows that a spawning pond is an indispensable adjunct to a successful hatchery. Further, as Professor Herdman shows, Mr. Fryer's argument fails to take account of the heavy risks of elimination to which breeding fish, ova, and fry are alike subjected in natural conditions. So convinced is

Prof. Herdman of these dangers that he believes that fish-hatching may be justified "even if not a single individual out of the millions of fry set free ever lives to become adult," for the additions of these millions to any area must greatly diminish the risk of destruction of the naturally hatched fry. There is, however, one point which Prof. Herdman does not mention which has been dwelt on by other critics, and which is again suggested by the results at Piel. As in many other cases it was found impossible to induce the larvae to feed. Even in the most successful cases they gradually diminished in vigour as the yolk disappeared, and ultimately died without ever having taken food independently. The curator suggests, as has been done by others, that it will probably be better another year to free the larvae before the yolk is completely absorbed. The obvious difficulty presents itself that there is no *proof* that the larvae are more likely to survive in the open sea than in the hatchery; some observers indeed believe that such larvae suffer from a form of anaemia likely to be as fatal in the open sea as in the hatchery. Will such larvae be of value even as food-supply to the enemies of the naturally hatched fry? On the whole it does not appear that we can go further than to say with Prof. Herdman that marine fish-hatching is still in the experimental stage. He believes, however, that the results so far obtained are sufficient to encourage workers "to continue the work vigorously in a hopeful spirit and with an open mind." There can, we think, be no doubt from the report before us that, whether the Laboratory be as yet economically justified or no, it certainly justifies its existence from the scientific standpoint, and though we know that the scientist is not concerned to show that his work has always a practical outcome, yet it may not be amiss to repeat that experience proves that one cannot tell where or when such practical outcome may be manifested. Indeed, the results here set forth of investigations on mussels and oysters, on plankton, on the food of young fishes, and so on, are laying a foundation upon which scientific aquiculture may one day be built.

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## "Symphily."

THE word "Symphily" may sound like a conundrum to many learned naturalists, but we are not responsible for more than the last letter of it. In its German form "Symphilie" it is used by Wasmann and others as a convenient term for the custom many ants have of showing liberal hospitality to various other insects. Unlike the cuckoos, who leave parental responsibilities to be discharged by proxy, not a few ants seem to have a positive mania for adoption. In fact, they indulge in hospitality to their own disadvantage, for at least some of the guests which are adopted in youth become curses to their foster-parents. In

this Wasmann finds an argument against the theory of natural selection which has fostered a custom or instinct of a disadvantageous sort. But this argument surely implies an altogether fallacious assumption, that this is the best of all possible worlds. No one can believe that natural selection has worked out an ideally perfect *modus vivendi* for any organism; what is worked out is a practicable compromise between the organism and the order of nature. A strong parental instinct has been evolved in ants; it is not perfect any more than any other parental instinct; it has the defects of its qualities; and the guests are really, as Escherich and Janet maintain, parasites on a virtue, illustrations, in short, of the familiar biological fact that there is no rose without its thorn.

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## The Jermyn Street Museum.

THE Government has decided not to remove the Museum of Practical Geology from Jermyn Street to South Kensington. There has been a good deal of feeling over the matter. For our part we think it would have been well to place the fossils at Jermyn Street alongside of those at the British Museum. It would have many advantages. To attempt to name fossils now, under the same conditions that prevailed half a century ago, is disheartening to any worker, and it seemed desirable that the Geological Survey should pay a little more attention to economic geology, for which purpose the Museum of Practical Geology was undoubtedly founded.

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## Science and Literary Style.

WE have received from Mr. Buckman a protest against the condemnation of his Homeric adjectives implied in our criticism last month of his article on Cycling. We admit that the question of the exact number of adjectives which the English noun may be induced to bear, is one which may be justly submitted to arbitration, but we still retain our original opinion that the specimens we quoted were instances of excess. It seems to us also too much that one poor substantive of five letters should be compelled to stagger under the weight of two such adjectives as "draggle-tail" and "limb-hampering," however justifiable these might have been in an earlier and more heroic age. Mr. Buckman—if we do not misunderstand him—is of opinion that the accumulation of adjectives, preferably of the compound type, conduces to intelligibility, and that this intelligibility has a price far above the rubies of literary style. We must admit that this opinion seems to be justified by much scientific writing of the present day.

Mr. Buckman says further that we have misunderstood him in some other respects, but we can do no more than refer our readers to the original paper which is now completed, and which will be found full of interest and instruction.

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## Welwitsch's Collection.

THE Trustees of the British Museum have just issued a third part of the account of the plants collected in Angola by Dr. Welwitsch. Like Parts I. and II. (already noticed in *Natural Science*) it is the work of Mr. Hiern, and carries the account of the Dicotyledons from Dipsaceae to Scrophulariaceae. Nearly half the book is occupied by the great order Compositae, of which 80 genera find representatives in the flora of this strip of western tropical Africa. According to Welwitsch's own estimate the proportion of Composites was in Angola proper about a sixteenth of the total number of species of seed-plants. Further south, in Benguella and Huilla, it was one-ninth or one-tenth, thus approaching nearer to that prevailing in the Cape flora, where the plants of this family reach their highest proportion. In the primeval forests of the mountains several arborescent species occur, and various small trees and shrubs supply the natives with tonic-bitter barks for use in cases of fever and diarrhoea. A species of lettuce in Loanda affords an excellent salad. By the incorporation of Dr. Welwitsch's copious notes the work has a general interest not usually associated with a technical systematic treatise. It will be news even to most botanists that the genus *Strychnos*, which gives us the deadly Nux Vomica, furnishes also "well-tasting fruits resembling oranges, and called by the natives Maboca," under which name several wholesome and common species of fruit are known, "especially in the interior of Huilla, where at their proper season, in December and January, the natives can buy from two to four dozen for a cotton handkerchief or a sheet of white paper."

In nomenclature Mr. Hiern follows Dr. Kuntze, and sometimes rejoices in outdoing his master. He has rediscovered several names of genera which by law of priority must replace those hitherto accepted. For instance, *Marsea*, *Detris*, *Elichrysum*, and the less euphonious *Crocodilodes*, all restorations from Adanson, will not be familiar to workers on Compositae, while *Pacouria*, founded by Aublet in 1775, must replace *Landolphia*, which did not see the light till early in the present century, as the name of the well-known rubber-producing genus.

The numerous specimens cited, the considerable proportion of new species described, and the great number of species hitherto known which are recorded, will impress the botanist with the untiring industry of Dr.

Welwitsch, his eminence as a careful and scientific collector and the great value of his collections, in the possession of which the British Museum may well feel proud. It is also matter for congratulation to all concerned that the parts of the catalogue follow each other at such short intervals, thus giving the promise of a speedy completion of the work.

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## American Mammalogy.

The first twenty-five pages of the January issue of the *Proc. Biol. Soc. Washington* (vol. xiii.) are occupied by papers on mammals from the pens of Messrs. Bangs, Merriam, Miller, and Palmer. Most of the matter in these communications is devoted to the never-ceasing descriptions of new species and sub-species, and the usual edifying controversies and arguments on nomenclatural questions. Mr. Miller's paper on the naked-tailed armadillos, for which the name *Tatoua* is stated to be the proper title, is, however, of more than ordinary interest; and perhaps some of our readers may like to know that such a familiar name as *Orca*, for the killer-whale, has, according to the strictest rules of nomenclature, no right to stand.

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## Flame Cells in Rotifera.

Mr. J. Shephard has written an interesting note on the structure of the vibratile tags or flame cells in Rotifera in the *Proceedings of the Royal Society of Victoria*, vol. xi. (N.S. pt. ii. p. 130). He describes the tag as a flattened funnel, closed at one end by a protoplasmic mass, to which is attached an undulating membrane, lying within the funnel, and two flagella as first seen by Rousselet (or one), playing freely outside in the coelom. He regards the appearance of the bundle or row of long cilia as due to a combination of the effect of the undulating membrane and the structure of the side walls of the funnel, which he describes as a striated membrane. Moxon, in 1864, noted the appearance of rows of waves passing along these side walls, and the appearance is familiar. Mr. Shephard notes, in addition, that by careful focussing a face view two such wave rows can be seen, one above the other; and this is his only evidence for the "striated" membranous wall. There is no necessity for this assumption. For if a row of cilia or undulating membrane vibrate, its face view will appear as a set of waves at the "loops" where it most closely approaches the wall, while the intermediate parts will be less visible; and if the optic angle of the lens be high enough to give a sufficiently thin optic section, the loops at the one side will come into view at a different

focus from those at the other. There is consequently no need to assume that the two layers of waves are due to two striated membranes.

Again, the question of undulating membrane or row of cilia is of no real importance; in a large number of cases undulating membranes have been proved to be simply agglutinated cilia, often separable on death. Of such nature are the comb-plates of Ctenophora, the membranellae of the disc in some Rotifers, and those of Ciliata. The distinction is not a strong one.

The details given are interesting and well put.

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## Plant Embryology.

IN the March issue of the *Botanical Gazette*, Prof. D. H. Campbell makes a further contribution to the embryology of the monocotyledons. A study of the embryo-sac of *Sparganium* reveals an enormous development of the antipodal cells after fertilization of the egg-cell. Hofmeister many years ago called attention to the conspicuous antipodal cells of certain grasses, and showed that the number might exceed the three usually found in angiosperms; and more recently Westermaier has pointed out that in the maize and others this exaggerated group of antipodal cells functions as endosperm in early stages of development of the embryo. Koernicke mentions thirty-six as an outside number, but Campbell finds in *Sparganium* more than a hundred cells, or in older stages a hundred and fifty. He suggests that these resemblances may imply a closer affinity than has hitherto been supposed between *Sparganium* and the grasses.

Another plant studied was *Lysichiton kamtschatcense*, an aroid inhabiting North-East Asia and the Pacific coast of North America. Here again the antipodal cells show an unusual development but in size rather than number. There are probably never more than ten, but they become extremely large and the nuclei reach enormous proportions. Their appearance is quite different from those of *Sparganium*, and indeed of any other known angiosperm; they most resemble certain Compositae such as *Senecio* and *Aster*, in which numerous antipodal cells have been described, and also a continuous endosperm, another feature of the embryology of *Lysichiton*. We cannot, however, regard these resemblances as indicating any systematic affinity. Professor Campbell has studied the embryology of other members of the order Aroideae and hopes soon to publish a full account of the results of his work.

# ORIGINAL COMMUNICATIONS.

## Evolution and the Question of Chance.

By F. W. Headley.

Professor Weldon has shown that in shore-crabs the dimensions of the carapace vary in accordance with the law of chance, and that the variations group themselves nearly symmetrically on either side of the mean. There is a large number of individuals slightly above the mean, a large number slightly below. Measurements and observations of other animals and of plants brought out similar results. If, therefore, an increase in any particular dimension is of advantage, there are many individuals in which this favourable variation is found, and, if it can be proved that their slight superiority makes the difference between survival and non-survival, then, it is maintained, all is plain sailing, and evolution pursues its way on strictly Darwinian lines. But is not this too large an inference to draw from the facts? Surely there still remains a great deal in evolution to be explained. Indeed, the main problem seems to me still to present itself without much alteration. It is one thing to explain the gradual enlargement or diminution of a particular organ, it is another to account for the various kinds of organs and for the various faculties that animals have developed. To take an instance, Prof. Weldon has proved that the frontal breadth of shore-crabs varies, and by experiment he has obtained evidence suggesting that the survival of an individual depends on its breadth in this region. But this hardly helps us to solve the problem why crabs have developed a chitinous exoskeleton instead of some other kind of cuticle. Are we here also to appeal to the law of chance and say that an unlimited number of kinds of external cuticle were available; that thousands of variations, each offering a different kind of integument, made their appearance; and that this particular kind of chitinous covering was selected in the case of crabs? It will be at once apparent that the law of chance as explained by Prof. Weldon—the tendency of variations to fall on either side of a mean—has no application in a case such as this when the question is one not of amount but of kind. It is true that all variations are due to chance, in the sense that we cannot

know the cause of any particular variation. Darwin, as may be seen by reference to the opening paragraph of the fifth chapter of the "Origin of Species," meant no more than this when he attributed variations to chance. He did not renounce the attempt to find law and system running through animal variability. For in his "Animals and Plants under Domestication," he writes at length upon the "laws of variation," upon "correlated variability," and on the "continuity of variation." He spoke of variations as "spontaneous," meaning that he could not trace them to a definite cause. But he strove to discover the laws which these spontaneous variations obeyed. Obscure as the subject is he has not left it in complete obscurity. The object of this paper is, if possible, to throw some further light upon it.

Even though we should be unable to discover any regulating principle, it is difficult—for me impossible—to allow free play to chance after tracing the development of a chick through its regular stages. Are we to suppose that the organism, roughly but truly, recapitulates its phylogeny, goes through a series of definite phases and at the end of the prescribed course is at liberty to break out into wild haphazard developments? Law and order so perfect surely never ended in chaos. Nevertheless, even if variations were chaotic with no guide but mere chance, if any animal might produce any kind of new organ, and if any existing organ might be moulded to any new form, even then, given an unlimited number of individuals in a species, mere chance might produce a sufficient number of representatives of some new type to form a new species. But the great results achieved by breeders of cattle, horses, pigeons, and other animals have been attained in many cases with quite small numbers. When breeding for some particular point, they have been generally able to find two animals among the few at their disposal in which this was prominent. The smallness of their resources and the greatness of their achievements put it out of the question that they have worked with nothing but chance to help them. If it were a question merely of more or less, then we might call in the doctrine of chances as expounded by Prof. Weldon. But how is the hood of the nun pigeon a question of more or less? In the wild rock dove or in the common dovecot pigeon there is no such growth of feathers in a rudimentary form. Game-fowls occasionally have as many as five spurs. In some breeds particular feathers show particular markings. In these cases we have new departures, not a gradual increase or decrease of an existing organ. Turning to wild animals we find problems of the same kind. There was a time when stags had no horns. The first development of these cannot be put down to deviations on either side of a mean in accordance with the law of chance. Before the horns originated, obviously there was no room for the working of the principle. When once they have come into existence, then their further development can be accounted for on the principle which Prof. Weldon has explained. Take a feather as

another example: it is a scale, but a scale that has been much divided and very highly elaborated. Is it possible to believe that by a process of mere addition a reptilian scale could become a feather? The plants from which our garden flowers are descended had themselves no flowers. The petals are no doubt leaves that have gathered in a whorl and changed their colour. But here again we have a change which cannot possibly be represented in terms of more or less. Parenthetically I may remark that I leave out of consideration the Lamarckian view. The idea that the crawling of bees or other insects over plants, or anything in the environment can have produced flowers is too great a strain on the credulity of an ordinary man. To me every acquired character is, as Weismann has put it, "simply the reaction of the organism upon a certain stimulus." Two more instances and I will leave this part of the subject. The whalebone that hangs from the whale's palate is a fresh development. It cannot be accounted for on Prof. Weldon's principle. In some fishes the air-bladder has been turned into a lung and an entirely new connection has been formed between it and a pair of aortic arches. Here, as in the preceding case, we have a new departure.

I am quite aware that if we hold that no discontinuous variation ever leaves its mark, but is swamped by inter-crossing, there is no such thing as a new departure. But discontinuity is a matter of degree. If we say that only infinitesimal variations can survive, we stultify ourselves, for such variations must be too small for natural selection to work upon. We must, therefore, recognise some amount of discontinuity, and the only question is, how much? I cannot here discuss the point at any length. But if, for instance, the growth of whale-bone was at the outset very minute, it is difficult to see how it could have been of any use, or have had selective value. In colour we know the shifts are sudden. There is a sudden appearance of albinism. Young domestic pigeons are sometimes quite different in colour from either parent. Wild-flowers when planted in a garden will often show some sudden conspicuous variation. However small the bloodvessels connecting the aorta with the swim-bladder turned into lungs, the connection at any rate must have been complete at the outset. For what would be the use of a vessel that communicated with nothing? However we may seek to minimise it, we must recognise some discontinuity, and in some cases some considerable amount of it. That being so, we recognise new departures. And if we recognise new departures, we cannot hold that evolution depends only on deviations from a mean.

If the law of chance fails us in many cases, where are we to look for guidance? I believe that Eimer has discovered where it is to be found. In his "Organic Evolution," p. 52, he says: "I assume with him (Nägeli) that the conditions for a progress towards the more complex, and towards division of labour, exists in the fact that

*a higher stage once reached can afford a foundation for one still higher*,[1] since the former, the existing stage, will necessarily be the starting-point for further modification." When reading his book some few years ago, I passed over this passage, failing to realise its importance. Now, having arrived at the same position independently, or, it may be, having recurred unconsciously to what I read there, I look upon the principle which Eimer enunciated as the clue to much that is otherwise inexplicable in evolution. We see everywhere in the organic world order and symmetry: hence it is difficult not to infer that the course of evolution has some better guide than mere chance. Some biologists find in external conditions, and in the creature's own activities, guidance sufficient. But the influence of the environment cannot shape a flower or thistle-down, or the samara of the maple. Nor in these cases can exercise have anything to do with the matter. We must find the guiding principle, then, in the organism itself, for there is no third alternative. Are we to say, then, that the first unicellular creatures contained in them *in parvo* all the complex animals and plants that have since arisen? That is unbelievable. But there remains Eimer's principle. As I understand it, it is this. Each step in advance decides within certain limits the next step. A few examples will make this plain. I take any genealogical tree, Haeckel's for instance ("Evolution of Man," vol. ii. p. 188), which is generally acceptable enough for our present purpose, though, no doubt, every biologist would prefer to draw out his own tree. Nearly at the base we find amoebae, one-celled organisms with a nucleus. Even at this very early stage there are very marked characteristics, notably cell-structure and multiplication by fission. If evolution is to proceed much further, there must be increase in size. Now the character which evolution has already impressed upon the amoeba allows of this only by one method. When fission takes place, the resultant cells may cling together, and then we have one of the Metazoa or multicellular organisms. Take now a point more than half way up the tree, where the Selachii, primitive fishes, are found. From these we find branching off the various kinds of fishes and the amphibians. Should the former course be followed there are paths leading towards the ganoids, the osseous fishes, and the mud-fishes. But development on amphibian lines, when once another alternative has been chosen, has become an impossibility. It is true there may be retrogression, but nature deprecates this, and even if a species retraces its steps it does not retrace them very far. This being so, every advance may be looked upon as a point gained. Every advance cuts off certain possibilities, and opens up certain others. The process may be compared to the system of case law, in which the interpretation of the law by a judge, at any rate if upheld on appeal, becomes binding on all judges who have to try similar cases. As we advance

[1] The italics are mine.

upward from the lowest forms of life, we see this principle constantly at work; each species is guided by its own phylogeny, by the course of its own development. Though it may deviate, it must not go beyond certain limits. Thus primitive reptiles had before them at least two possibilities—avian and specialised reptilian lines of development. A species of bird, if it develops further, must modify the avian type, or the type of its own particular order, but it cannot become a reptile. The amphibian type is still further beyond the region of possibility. As we ascend, it will be seen that the lines along which species may develop, though certainly not less in number than lower in the scale, are less divergent one from another. A primitive bird was evidently capable of evolving an enormous number of specialised forms, but the distance between these is as nothing when compared with the distance between the descendants of primitive Protozoa. The more elaborated and complex the species, the firmer the hand with which its own phylogeny guides it.

In describing this limiting principle I may have seemed to be speaking of known forms as the only possible ones. I am far from thinking that. The known species, living and fossil, may be far less in number than those of which we have neither record nor living representatives. I only maintain that *at any particular stage in the evolution of a species the paths open to it are limited in number*. This sets no limit to the number of forms that may eventually arise. For each advance not only leaves behind a number of possibilities, but opens up a number of others. The particular number of variations possible at any stage we cannot of course discover. But there are the following reasons for believing that they are not very numerous. We have seen that for one-celled organisms the only possible way of advance in size is by the coherence of the resultant cells after fission. When we come to the more complex animals we know their various parts are tied together on the principle known as correlation. The subject of correlation is very imperfectly understood. What is known of it enables us to see that the organism is no mere aggregate of units, but that a variation in one part involves a variation in several others. Hence the organism's capacity for variation does not increase so fast as its complexity increases. Of the limitation which we must attribute to correlation there is abundant evidence. There is the experience of breeders already mentioned. They have made many new breeds, and often with only small numbers to select from. To pass on to other evidence of limitation we find among evolutionists a growing tendency towards the view that species, and even larger groups, not seldom tend to converge. In the Arthropoda it is probable, or at anyrate possible, that we have instances of convergent evolution. There is reason to believe that the ostrich and its allies illustrate the same principle; they are probably polyphyletic in origin, and have converged towards one another. The dentition of the marsupial animals, according to

their food and habits, approximates to that of placental mammals of similar habits. The heart of a bird with its four chambers is strangely like the heart of a mammal, and the difference between the avian and mammalian valves between the two right-hand chambers only serves to bring out the similarity of the main architectural features. Here, then, we have two undoubted examples of convergence; several other highly probable instances have already been mentioned. Is such convergence likely, if the number of possible variations is unlimited? In this connection much might be said on the subject of what Darwin called "analogous variations," the phenomenon of two allied species varying in the same way.[1] In some cases it may be possible to see the actual limit of the capacity to vary. I cannot help thinking that we know all the kinds of outgrowth of which the epidermis is capable; that hair, wool, bristles, teeth, the various formations of horny material, scales, and feathers, are the only possibilities. I infer this from the elasticity of the environment—a subject to which I shall soon return—which allows an organism to choose its own method of adaptation. I would set no limit to the possible shapes and positions of such epidermic growths. I am speaking only of the kinds of material available.

Lastly, I would call attention to the *continuity of variation*, to use Darwin's term for it: the known likelihood that among the offspring of parents in which a particular variation has occurred some will vary in the same direction, so that if a flower shows a tendency to doubleness, some of the seedling plants from it are likely to show the same tendency in a greater degree. There are two main principles, then, on which I wish to insist—(1) that there is at any stage in evolution a limit to the number of possible variations, (2) that there is continuity in variation.

The establishment of the former of these two principles introduces another difficulty. If any variation that appears in any animal must be in conformity with the organisation of the animal in question, and if the number of possible variations is thus limited, how is it that adaptations to environment are as perfect as they are? It is the constant cry of Lamarckians that variations are all definite, *i.e.* not vague tentative deviations, but unmistakable adaptations. I cannot myself accept this proposition when thus broadly stated. But, recognising that adaptation is the rule in nature, I wish to show how a consideration of the character of the environment will help to remove the difficulty.

The environment offers to animals all that they require, and lets them take what they want in any way that they may choose. They must have oxygen, but they may get it by whatever means they like. They must have protoplasm for food, but they may adopt any means

[1] See "Animals and Plants under Domestication," vol. ii. p. 348; see also Eimer's "Zoologische Studien auf Capri."

of laying hold of it that suits their own organisation. The contrivances for seizing food are many and various, and with none of them does the environment find fault. For this purpose most mammals use their teeth, the "snake-handed" elephant uses his trunk, the bird his beak and supple neck. The boa constrictor's whole body is used for grasping. The lobster has his claws, and for man there is his hand, that almost perfect grasping implement. The environment is so accommodating that almost any good piece of machinery works well there. When we see a *Kallima* butterfly we are apt to think of the environment as a procrustean mould into which it has had to fit itself: the insect has had to become, we may imagine, like a decaying leaf, or perish. This is true, no doubt, of the individual *Kallima* now. It is not true of the species long ago before it had advanced any distance along its line of development. At that distant date it might have maintained itself by resemblance, not to decaying leaves, but to something else, by "mimicry" of some other species protected by its nauseating flavour, by itself developing a nauseating flavour, by the vigour of its flight, or by its great fertility. In this particular butterfly we must imagine some slight accidental resemblance to a dead leaf at the outset. The perfection of adaptation is due to its proceeding along the line of development it has once adopted. In each generation those were eliminated which were not in this particular point up to the standard of excellence.

In many instances of adaptation there is much to wonder at. There is, on the one hand, the marvellous variability of living organisms, due to causes which we can only speculate upon, and on the other there is the easy-going elasticity of the environment which can find places for so many different types. The variations that an organism may develop are determined by its own character, which character has been built up in the long course of its phylogeny. When a species has proceeded far along its particular line, then the environment may seem to be an exacting tyrant, allowing of only two alternatives—extreme specialisation in one direction, or extinction.

It is not, however, the environment, but the species, which is to blame. It has lost its plasticity. Its own phylogeny compels it to follow a certain mode of life. The environment is as accommodating as ever, but the organisms that make up the species claim to maintain themselves in one way and no other. And such a demand a changing environment may sometimes refuse.

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# Some Recent Work on the Anatomy of Fossil Plants.

By Arthur J. Maslen, F.L.S.

No plant types are more interesting, and certainly none have yielded more valuable results to the palaeo-botanist during the last few years, than the numerous forms now known which present intermediate characters tending to unite the ferns with the cycads.

These synthetic forms (of which, unfortunately, the reproductive organs are in no case known), have recently been made into a group—the Cycadofilices, and this again may be conveniently divided into two fairly distinct series; the first including such types as *Lyginodendron*, *Heterangium*, *Poroxylon* and *Protopitys*, in all of which the stem is essentially monostelic (containing only a single vascular axis); and the second containing a series of forms, the stems of which exhibit a more or less complicated polystelic structure, well seen in the most familiar forms—the Medulloseae.

The two most interesting monostelic forms, *Lyginodendron* and *Heterangium*, are common in the calcareous nodules of the English Coal Measures, and have formed the subject of the last of the valuable contributions to palaeontology made jointly by the late Professor W. C. Williamson and Dr. D. H. Scott.[1] The material with which these two botanists were able to work was so good, and the preservation of the vegetative organs so perfect, that it was possible to work out the structure in great detail, and to arrive at such certain conclusions as would astonish the botanist unacquainted with modern palaeo-botanical research. To such the illustrations in this memoir (and in others) will be a revelation.

The generic name, *Lyginodendron*, was originally applied by Gourlie to casts of stems exhibiting what is now generally known as the "dictyoxylon structure." This structure, which consists of the presence in the outer cortex of a strengthening tissue, composed of an anastomosing longitudinal network of sclerenchymatous strands, leads to a very

[1] "Further Observations on the Organization of the Fossil Plants of the Coal-Measures. Part III. Lyginodendron and Heterangium," *Phil. Trans.* (1895), vol. clxxxvi. B. pp. 703-779, pls. 18-29.

characteristic appearance in some of the casts; but, inasmuch as later research has shown that the same structure is common in the outer cortex of many quite distinct Palaeozoic plants, the name *Lyginodendron* lost its generic significance until Williamson adopted it for some specimens showing internal structure, which he believed to be identical with some of the casts to which the name had been originally applied.

Unfortunately, as already mentioned, nothing whatever is known as to the organs of fructification of any of these forms, or, at least, if they are known they have never been correlated with the vegetative organs. "So long as the mode of reproduction is unknown, it will be impossible to assign these genera definitely to their systematic position; in the meantime, we can only weigh with due care such evidence as is afforded by their vegetative structure. This evidence, as we shall show, clearly indicates, as far as it goes, a position intermediate between ferns and cycads."[1]

However, the taxonomic value of anatomical characters is gradually becoming more and more recognised. Dr. Scott, in his Presidential Address to the Botanical Section of the British Association in 1896, referring to this, instances the Marattiaceae as an example, pointing out that in this old fern-family there is great uniformity in anatomical structure, while the sporangia show the important differences on which the distinction into genera is based.

Another good example may be found in the Calamites, the structure of which has been worked out with very great completeness by Williamson; and later, by Williamson and Scott.[2] These later researches have shown that the primary structure of these common Coal Measure fossils, is essentially so exactly that of an *Equisetum*, as to leave no question as to their near relationship, and they have accordingly been placed in the Equisetales. And yet, so recently as the publication of Solms-Laubach's "Fossil Botany" in 1887, we find the Calamites treated of in quite a distinct part of the book from the Equisetaceae, recent and fossil. A most interesting historical sketch of the growth of opinion as to the affinities of the Calamites will be found in the recently published valuable introduction to the botanical study of fossil plants by Mr. A. C. Seward.[3]

Returning to the Lyginodendreae, in *Lyginodendron* the stele of the stem consists of a central pith, around the periphery of which are a number of distinct strands of primary xylem, external to which is seen, in most cases, a broad zone of secondary wood, the elements of which are arranged with great regularity in radial rows. The primary bundles are distinctly collateral in structure and exhibit a distribution of their elements, which is of the highest importance for comparison with

[1] *Loc. cit.* p. 704.

[2] "Further Observations, etc. Part I. Calamites, Calamostachys, and Sphenophyllum." *Phil. Trans.* (1894), vol. clxxxv. p. 863.

[3] "Fossil Plants," Cambridge University Press, 1898, vol. i. pp. 295-302.

modern forms. It is characteristic of the primary vascular bundles in an ordinary stem for the xylem to differentiate entirely in a centrifugal manner, *i.e.*, for the first formed elements (protoxylem) of the primary xylem to occupy the inner limit of the bundle. But the examination of sections of the stem of *Lyginodendron* at once shows that the smallest first-formed elements of the xylem, lie neither at the outer nor at the inner limit of the primary xylem, but occupy an intermediate position, nearer the outer than the inner side. This being so, it follows that the primary wood has developed both outwards (centrifugally) and inwards (centripetally) from the protoxylem. To this type of structure Solms-Laubach has applied the term mesarch, while Williamson and Scott describe the same structure as mesoxylic.[1]

The interest of these mesarch collateral primary bundles in *Lyginodendron*, lies in its comparison with living forms. Until quite recently mesarch bundles were believed to be peculiar to the foliar bundles of Cycadeae, the stem bundles being of the ordinary type. However, in a recent paper in the *Annals of Botany*, by Dr. Scott, we are told that—"The frequency of mesarch structure in the stems of Palaeozoic plants showing unmistakable affinities with Cycadaceae, renders it highly probable that this character formerly extended to the stem, as well as the leaf, of the Cycadaceae themselves. This consideration suggested a renewed investigation of the anatomy of recent cycads, in order to ascertain whether some vestiges of mesarch structure might not still survive in the bundles of the stem."[2] Of the axial organs of cycads, however, only the peduncles of certain forms (*Stangeria paradoxa, Bowenia spectabilis, Zamia Loddigesii, Ceratozamia mexicana, C. latifolia*) were found to still exhibit the structure in question.

The secondary xylem of *Lyginodendron* (which is present with the rarest exceptions in all known stems) was formed by a normal cambium forming wood internally and phloem externally, and in well-preserved specimens the actual cambium cells are quite evident. In general characters the secondary xylem is quite similar to that of the stem of cycads; it consists of pitted tracheides and ray-parenchyma only, and the xylem is split up into vascular plates only a few tangential rows in thickness by the numerous medullary rays.

Numerous structural anomalies in the stems of *Lyginodendron* have been noted by the authors in the rich material with which they have worked. The most interesting of these variations is that of the appearance of a secondary meristem on the outer border of the pith, and giving rise to wood and phloem with inverted orientation, *i.e.*, with xylem outside and phloem inside. This type of anomaly is exactly that found in the stems of some modern plants, such as *Tecoma grandiflora*, a root-climber belonging to the Bignoniaceae, and its

[1] *Loc. cit.* p. 713.

[2] "The anatomical characters presented by the peduncle of Cycadaceae," *Annals of Botany*, vol. xi. p. 401.

occurrence in *Lyginodendron* is regarded by the authors as merely an occasional variation of no specific value. This point is taken as a peg on which to hang a few general remarks on the *indiscriminate* use of anatomical characters. They point out that—"The anomaly in question is known to be of very inconstant occurrence at the present day. Both in the genera *Tecoma* and *Iodes* some specimens show it and others do not, though analogous peculiarities (internal phloem for example), are often characteristic of entire natural orders. Anatomical characters, in fact, like any other characters, are sometimes of great constancy, sometimes highly variable, while the same character which is relatively constant in one family may be most inconstant in other groups." [1] They further point out that—"In the present paper, we have been compelled, in the absence of organs of fructification, to make great use of anatomical characters. We have, however, endeavoured to rely on those which are known to be of great persistency in families which presumably belong to the same cycle of affinities as the plants with which we are dealing." [2]

The leaf-traces, which pass out from the stele of the stem so very gradually as to have led Williamson, at first, to think they were cauline bundles, have been shown to be continuous with the peri-medullary strands (primary bundles) around the pith, and many sections show these bundles passing through the secondary wood on their way out to the leaves.

It is a rather remarkable fact that, in all the specimens of *Lyginodendron* which have hitherto been examined, no trace has been seen of branching. Of course the evidence is not conclusive, but not a single branching specimen has yet been detected, since all the supposed branches which have been described have turned out to be either petioles or adventitious roots.

With regard to the petioles of *Lyginodendron*, Williamson long ago surmised that these were identical with his *Rachiopteris aspera*, and the discovery of the leaflets in connection with the branched petiole led to the conclusion that the leaf can be referred to Brongniart's form-genus *Sphenopteris*. We therefore have here the combination of cycad-like stem and distinctly fern-like leaves. But the authors point out that something more is needed to establish filicinean affinities: "The classical case of *Stangeria* is a sufficient warning against any such hasty inference. It must, however, be remembered that in the foliage of *Lyginodendron* we have not only fern-like form and venation, but also fern-like *structure*, whereas in the case of *Stangeria* a single transverse section of the petiole would be sufficient to prove that the plant is no fern, but a cycad." [3] A striking example this of the importance of a knowledge of the internal structure of plants in these investigations! The bundles in the petiole are the typical concentric ones of a fern, while the continuations of the same bundles in the stem are collateral, and have a distinctly cycadean character.

[1] *Loc. cit.* p. 722. [2] *Loc. cit.* pp. 722-723. [3] *Loc. cit.* p. 727.

It should be pointed out, however, in this connection, that Dr. Scott, in his recent paper on "The anatomical characters presented by the peduncle of Cycadaceae," already quoted, has shown that in the peduncle of *Stangeria paradoxa*, T. Moore—which shows the mesarch structure and consequent development of both centripetal and centrifugal primary xylem—the small cauline cortical bundles which also occur in the peduncle sometimes exhibit a typically concentric structure. These concentric bundles are of considerable interest, not only as the only recorded cycadean bundles with a primarily concentric structure, but also as probably indicating a recurrence to a condition which appears to be a relic of an ancestral fern-character. To quote Dr. Scott: "The evidence for the origin of cycads from ferns is now overwhelming, Professor Ikeno's discovery of the multiciliate spermatozoids of *Cycas* completing the proof in the most striking manner." [1]

Still more recently Mr. Worsdell has described "The vascular structure of the sporophylls of the Cycadaceae," [2] and has shown that, exactly as in the case of the peduncle, the sporophylls borne on it retain certain primitive characters which are not found in the foliage leaf. He shows that this primitive character is exhibited here also by the development of concentric bundles which occur in both fertile and barren sporophylls in several genera. "These concentric bundles are absent from the foliage leaves, the structure, number, and orientation of whose bundles are extremely regular, constant and well defined, whereas in the sporophylls the reverse is the case, a fact which probably points to their possessing a more primitive structure, viz. one not so perfectly adapted and stereotyped to subserve a special physiological function, as is the case with the foliage leaves." [3]

On the outer surface of the cortex in both petiole and stem of *Lyginodendron* are seated some curious emergences, the structure of which suggests a gland of some kind. It was the similarity of these structures on stem and leaf-stalk which first led Williamson to surmise that the petioles (*Rachiopteris aspera*) were borne on *Lyginodendron* stems. Similar appendages are found on some recent tree-ferns, more especially on *Alsophila australis*.

That the fossils previously described by Williamson as *Kaloxylon Hookeri* are the adventitious roots of *Lyginodendron* appears to have been proved almost simultaneously and independently by Williamson and Scott and by the late Mr. Hick.[4] In the memoir by the two first authors the evidence on which this conclusion is based, and the somewhat peculiar structure of these appendages is fully described. That they are really roots is proved not only by their primary structure, but

[1] *Loc. cit.* p. 409, and *Annals of Botany*, June 1897.

[2] *Annals of Botany*, June 1898. [3] *Loc. cit.* p. 239.

[4] "On *Kaloxylon Hookeri*, Will., and *Lyginodendron Oldhamium*, Will.," *Mem. and Proc. Manchester Lit. and Phil. Soc.* (1895), vol. ix. p. 109-116.

also by their endogenous origin; tangential sections through the cortex of the parent plant, and passing also through an adventitious root, showing the latter as a complete organ possessing a cortex and epidermis of its own, whereas a similar section through the base of a branch would show only the vascular tissues passing outwards from the stele of the main stem. The authors point out that—"The typical roots of *Lyginodendron*, before the commencement of secondary growth, often bear a striking resemblance to the smaller roots of recent Marattiaceae." [1]

The roots exhibit secondary thickening in a quite normal manner, but curiously enough become more like certain dicotyledonous roots (e.g. *Cucurbita Pepo*) than those of cycads or ferns. The peculiar and characteristic appearance presented by the older roots, in which the secondary xylem shows a peculiar disposition, generally into five or six large wedges, separated from one another by wide parenchymatous wedges, is simply due, as also in the *Cucurbita* mentioned above and figured by De Bary,[2] to the fact that, whereas opposite the primary phloem groups great masses of xylem separated by narrow medullary rays are formed, thus building up the wedges of wood, opposite the primary xylem the cambium forms mainly parenchyma.

All the specimens of *Lyginodendron*, of which the structure is described by Williamson and Scott, are comparatively small in size (not exceeding about 4 cms. in diameter), while many of the cortical impressions, if they really belong to *Lyginodendron*, represent plants which must have attained the dimension of trees. Mr. A. C. Seward has described, under the name of *L. robustum*,[3] the structure of a much larger specimen, of which the diameter of the woody cylinder and pith alone attains 14 cms. The preservation of this specimen is not as good as in *L. Oldhamium* (the ordinary form), but traces of the primary bundles can be seen, as also an internal ring of centripetal xylem, similar to what has been described in *L. Oldhamium* as anomalous wood. The author points out that there is great similarity between *L. robustum* and a type from Autun, described in 1879 by Renault as *Cyeadoxylon Fremyi*, and adds that "the term *Cyeadoxylon* proposed by Renault, if extended in its application, might serve as a general generic designation for plants possessing secondary xylem closely resembling that of recent Cycads." [4]

In the species of Permo-carboniferous *Poroxylon* described by MM. Bertrand and Renault,[5] we find a similar arrangement of parts to that in *Lyginodendron*. They too must have formed small trees. As in

[1] "Further Observations," etc. Part. III. p. 738.

[2] "Comparative Anatomy of the Phanerogams and Ferns," English Edition, p. 474, fig. 204.

[3] "A Contribution to our knowledge of Lyginodendron," *Annals of Botany*, March 1897.

[4] *Loc. cit.*, p. 84.

[5] "Recherches sur les Poroxylons," *Arch. bot. du Nord de la France*, 1886.

the latter genus, the vegetative characters are very completely known, and they approach the cycads even more closely, since here the cycadean type of vascular bundle is found, not merely in the stem but in the petiole also. Moreover, in *Poroxylon* secondary wood was formed in the leaf, and what is still more interesting is the fact that the latter organs (the leaves) appear to approach the familiar Coal Measure forms of *Cordaites*. On this point Dr. Scott points out that *Poroxylon* is "known only from the Upper Carboniferous, so we cannot regard it as in any way representing the ancestors of the far more ancient Cordaiteae. The genus suggests, however, the possibility that the Cordaiteae and the Cycadeae (taking the latter term in its wide sense) may have had a common origin among forms belonging to the filicinean stock." [1]

MM. Bertrand and Renault, however, do not recognise filicinean affinities in *Poroxylon*. "Ils n' ont aucun rapport avec les Ptéridophytes," and they attempt to derive not only *Poroxylon* but *Lyginodendron* and the cycads themselves from the lycopods. But, as Dr. Scott again points out, this view is no longer tenable. Both recent cycads and their fossil allies (Bennettiteae, Lyginodendreae, Medulloseae) abound in fern-like characters, and have simply nothing in common with Lycopods. . . . Recent work has completely confirmed Williamson's original view as to the fern-affinities of these fossil genera." [2]

Count Solms-Laubach has also described the structure of a somewhat curious ally of *Lyginodendron* from the Culm of Falkenberg, under the name of *Protopitys Buchiana*.[3] Here the plant bore distichous leaves, and the central cylinder has the form of an ellipse composed of a large pith surrounded by a thin continuous ring of primary wood, the whole being surrounded by a considerable thickness of secondarily developed xylem. From the extremities of the long axis of the ellipse the petiolar bundles are given off alternately from either end. Solms-Laubach himself considers this type as representing a special group—the Protopityeae, but he is in general agreement with Williamson and Scott in thinking that *Protopitys* constitutes another of the types which are intermediate between the Filicineae and the gymnosperms.

Returning now to the genus *Heterangium*, founded by Corda in 1845, and which has a wider range in time than *Lyginodendron*, and of which the best specimens have been obtained from the Burntisland beds at the base of the Carboniferous system, we have a type which at first sight seems very different from *Lyginodendron*, but which, on closer

[1] *Report British Assoc.*, 1896 (Pres. Add. Botanical Section), p. 1008.

[2] "The Anatomical Characters presented by the Peduncle of Cycadaceae," *Annals of Botany*, vol. xi. pp. 415, 416.

[3] "Ueber die in den Kalksteinen des Kulm von Glätzisch-Falkenberg in Schlesien enthaltenen Structur-bietenden Pflanzenreste," II. Abhandlung. *Botan. Zeitung*, 1893, pp. 197-208.

examination, shows many points of similarity, although its characters are altogether more filicinean. Williamson and Scott describe in detail two species (*H. Grievii*, Will. and *H. tiliaeoides*, Will.),[1] of the former of which we now have the most complete knowledge, while the detailed preservation of some parts of the latter is simply marvellous.

This old type differs from *Lyginodendron* mainly in the primary structure of the stele of the stem. There is no pith, but the whole of the interior of the stele is occupied by the primary wood, consisting of groups of tracheae interspersed with irregularly arranged bands of parenchyma. The stem is therefore in general structure similar to that of a typical monostelic fern such as *Gleichenia*. Unquestionably *Heterangium* is much nearer the ferns than is *Lyginodendron*, but it is most interesting to find, when we pass to the structure of the strands of xylem which occupy the periphery of the primary wood, that here again the primary bundles are mesarch, an arrangement which we have already seen to be a cycadean one.

In this case, again, as in *Lyginodendron*, most of the specimens show a zone of secondary wood, which is, however, on the whole, less developed than in the latter genus. The petioles and leaves (which have been identified with *Sphenopteris elegans*) also appear to have been of the same fern-like type, both in form and structure, as those borne on *Lyginodendron*. The root was also quite of the "*Kaloxylon*" type.

In *H. tiliaeoides*, Will., from the Halifax Coal Measures, which received its specific name on account of a resemblance in some points of its anatomy with the common lime-tree (*Tilia*), the tissues are most beautifully preserved. Anatomical details, such as the cambium elements and the sieve-tubes of the phloem, are here preserved, perhaps better than in any other known fossil; in some specimens even the sieve-plates can be recognised with certainty.

Both in *Heterangium* and in *Lyginodendron*, fern-sporangia have been found in association with the remains, though never in actual continuity with them. Nothing in the nature of a cone has ever been found. The authors incline to the view that the material investigated consists entirely of young plants, and in that way seek to account for the absence of fructification; considering that the whole question of the nature of the fructification of these forms is best left open.

All the preceding types have agreed in one important character, that of monostely, there being but one vascular axis in the stem. There is, however, an equally important series of polystelic forms now known to belong to this intermediate group of Cycadofilices. These are mainly known from foreign specimens, most of which exhibit a very complicated stelar structure, and have been known for some years, although Dr. Scott has quite recently described an interesting and very much simpler form (*Medullosa anglica*) obtained from the Lower Coal Measures of this country.

[1] "Further observations, etc." Part III., *loc. cit.*

Most of these types have been put into the class Medulloseae, some of the forms belonging to which were, in the very early days of palaeontology, regarded as Carboniferous palms. Mr. A. C. Seward, in a paper on *Myeloxylon*[1] (now known to be petioles of *Medullosa*) points out that Corda thought it a palm, and Brongniart a doubtful monocotyledon; but the researches of Goeppert, Williamson, Weber, Sterzel, and others have shown that the affinities are both cycadean and filicinean. On the whole, Mr. Seward concludes that the fossil specimens are related more nearly to the cycads than to the ferns.

Sterzel and O. Weber[2] made a very important addition to our knowledge of the Medulloseae by proving that these petioles (*Myeloxylon*) were borne on stems of *Medullosa*. The petioles in transverse section exhibit a large number of vascular bundles encased in a mass of parenchyma, each separate bundle being collateral and having the protoxylem placed on the side turned towards the phloem, the xylem having therefore developed entirely in a centripetal direction, an evidently cycadean character. Sterzel also regards the Medulloseae as having more affinities with cycads than with ferns.

The Medulloseae exhibit, in their stem, a type of structure different from anything known among living plants, and probably represent, as Solms-Laubach has suggested, a divergent branch which has left no descendants among the existing vegetation. They range from the Coal Measures to the Permian strata, and vary considerably in the complexity of their internal stelar structure; the outer surface of the stem being, in most cases, unknown.

The oldest and simplest known form of the genus, *Medullosa anglica*, from the Lower Coal Measures of Lancashire, has been recently described before the Royal Society by Dr. Scott.[3] "These fossils are of special interest on several grounds: they are considerably more ancient than any members of the genus previously described; they are the first English specimens recorded; they are preserved in a more complete and perfect form than any others at present known; and lastly, the greater simplicity of their structure causes the essential characters of the genus to stand out with greater clearness than in the more complex species." In some of the specimens the habit of the stem, clothed with overlapping leaf-bases, can be made out, and appears to have been not unlike some of the tree-ferns, such as *Alsophila procera*.

The transverse section of the stem shows the presence of three or four steles, each possessing a pithless xylem strand with the protoxylem elements close to, but not actually at, the periphery (mesarch). Each

[1] *Annals of Botany*, 1893, vol. vii.

[2] Sterzel, J. T. D. "Beiträge zur Kenntniss der Medulloseae. Nach Mitteilungen und älteren Abbildungen von O. Weber nachträglich bearbeitet," *Ber. Nat. Ges. Chemnitz.* (1896), xiii. pp. 44-143.

[3] See *Nature* (1899), vol. lix. p. 381.

individual stele in fact has almost precisely the same structure as the single stele of *Heterangium*, and like the latter develops a cambium and secondary wood and bast, the xylem being of typically "loose" cycadean nature. The leaf-traces, which pass outwards from the stelar system to pass into the leaves, carry with them some secondary wood. As it passes through the cortex, however, the trace loses its secondary tissues and undergoes splitting into a number of bundles, each of which is collateral in structure. The base of the leaf therefore received a large number of bundles, and this distribution of the bundles is described as peculiar and unlike that in any known plants of cycadean affinities.

In other species of *Medullosa* the structure is more complex, there being usually a number of small circular steles occupying the central region of the axis, and one or more larger wavy or annular steles or rings of steles, closer to the periphery, and from which the leaf-traces appear to arise. All the steles exhibit secondary thickening.

A very interesting suggestion in connection with the structure of *Medullosa* has been made by Mr. W. C. Worsdell in his paper on "The anatomy of the stem of *Macrozamia* compared with that of other genera of Cycadeae."[1] The stem of *Macrozamia* (in common with those of *Cycas* and *Encephalartos*) possesses a number of anomalous rings of collateral bundles, secondarily formed outside the normal zone. The tissues in these zones show the normal orientation, having xylem inside and phloem outside; but there is frequently found to be present, either between the normal and the first anomalous ring, or between two anomalous rings, a cambium which develops small bundles with reversed orientation, *i.e.* with xylem outside and phloem inside. In some cases, where the inverted segment lay rather obliquely, it formed an almost continuous zone with the normally orientated segment, thus indicating a tendency towards the formation of a concentric bundle. If these bundles were more completely developed the anomalous zone would be converted into something like what we find in *Medullosa*, steles with xylem internal and phloem both on the inner and outer side. The author says: "The whole structure, both of the anomalous zones and the tertiary cambiums, recalls strongly that of the stem of the Medulloseae, a fossil group with many cycadean affinities; but it would be rash and premature to suggest here a homology between the two." He gives the suggestion, however, that these anomalous structures are the remnants of some ancient structure once common to a large number of plants, and that this structure consisted of rings or layers of concentric vascular stands.

Evidence has been brought forward by Renault and others to prove that the "*Myeloxylon*" petioles bore distinctly fern-like foliage, referable to the form-genera *Neuropteris* and *Alethopteris*. Sterzel thinks that *Callipteris*, *Alethopteris*, *Odontopteris*, and *Neuropteris* are

[1] *Annals of Botany*, 1896, vol. x. p. 601.

fronds of Cycadeae rather than of ferns, the absence of fructification lending some support to this view, while Dr. Scott points out that many of the most characteristic forms of so-called fern-fronds from the Carboniferous rocks cannot be accepted as true ferns, but really belong to these intermediate synthetic forms.

We thus see that palaeo-botany, now that it is entering on a stage of its development in which anatomy is taking a leading place, has already yielded positive results, the value of which is only beginning to be recognised by botanists in general. The value of the direct evidence thus obtained may be trusted to throw much light on the phylogenetic history of the groups of living plants, and in no instance is this better seen than in relation with the Cycadeae. That the modern cycads represent an offshoot of a very old cycado-filicinean stock there is no longer any doubt, although the direct links in their phylogenetic history are by no means easy to make out; and this difficulty will remain until much more palaeontological evidence is forthcoming.

In anatomical characters the modern cycads show great variation, and appear to retain many primitive characters, the meaning of some of which is now to a certain extent known. As Dr. Scott points out: "The Cycadales may even be polyphyletic, having perhaps sprung from the Filicineae at various points. The Lyginodendreae, Protopityeae, and Medulloseae all combine Filicinean with Cycadean characters, but they appear to join on to very different groups of ferns."

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# A Recent research on Epitokous Forms of Annelids.[1]

By Professor W. C. M'Intosh, M.D., F.R.S.

Few subjects in comparatively recent years have excited more interest than the remarkable changes which Prof. Ernst Ehlers of Göttingen first pointed out in the Nereids at the reproductive period. The worms in which these changes occurred were called by Ehlers epitokous forms, while Prof. Edouard Claparède of Geneva termed them epigonous.

Messrs. Maurice Caullery and Felix Mesnil, respectively of Lyons and Paris, have described, in an important recent paper, the results of their observations on a group hitherto unsuspected of harbouring such features, viz., on the Cirratulidae, and more especially on the epitokous phases of *Dodecaceria concharum*, Oerst., a species much more abundant in the south than in the north, probably because the calcareous rocks in which it bores are more characteristic of the French shores and those of southern England, and the growths of *Lithothamnion*, into which it likewise perforates, more luxuriant in these genial waters.

Before entering on the special subject of the epigamy of the Cirratulidae, it may conduce to perspicuity if we take a brief survey of the groups in which these remarkable sexual phases are at present known. No epitokous forms have hitherto been met with in the Amphinomidae, including, for the most part, somewhat sedentary annelids like *Euphrosyne* and *Spinther;* nor is it a prominent feature in the Aphroditidae, the only types that can be mentioned in this connection being *Dreischia* and *Nectochaeta*, pelagic forms the life-history of which is not yet sufficiently known to warrant accurate deductions on the subject. It is possible they are allied to such as *Evarne hubrechti* and *Evarne johnstoni*, forms in which the bristles are much elongated and, in certain examples, the eyes considerably enlarged. Like some other groups of annelids, moreover, the young stages of the Polynoidae are characterised by greatly elongated bristles during their pelagic life, but no reliable deduction as to the connection with the epitokous condition in the adult can be drawn from this feature.

No example of epigamy is known in the Nephthydidae, yet at St. Andrews a form of the common *Nephthys caeca*, O. Fabr., shows a

[1] Caullery and Mesnil, "Les formes épitoques et l'évolution des cirratuliens," 8vo, pp. 200, with 6 plates; Paris, 1899, price frs. 7.50.

characteristic elongation of the bristles in summer, and is found amongst the rocks rather than amongst the sand. So different is the aspect of this form that it has been described as a different species.

In the Phyllodocidae Malmgren describes a *Eulalia* (*E. problema*), from Greenland, in which the middle and posterior feet have greatly elongated simple bristles. De Saint-Joseph mentions two ripe females of *Mystides limbata* which, besides the ordinary bristles, had very long tufts of simple bristles in the middle of the body and also enlarged eyes, absent in unripe individuals. A similar condition is seen in the *Eulalia gracilis*, Verr., of Webster and Benedict. The large-eyed species described by the writer in the "Challenger" volume, however, gave no evidence of a pelagic habit.

In the Hesionidae Malaquin indicates that an epitokous condition occurs in *Kefersteinia cirrata*, and Webster and Benedict describe *Hesione gracilis* as bearing very long dorsal bristles.

The Syllidae, again, offer well-marked examples of epigamy, some at sexual maturity assuming the large eyes, long natatory simple bristles and other characters. In certain forms it is the males only which are affected, in others the females, in which the long natatory bristles are shed after the escape of the eggs. Moreover, these bristles are confined to the posterior region of the body in which the reproductive elements are developed in the form of stolons. In *Autolytus* and allied forms the stolons so differ from each other as to have been included in different genera. An *Exogone*, again, is described by Malaquin which presents now epigamy, now schizogamy (separation of buds). Haswell found a *Syllis* which, while showing eggs anteriorly, developed sperms posteriorly, the latter region developing a head and swimming away as an independent form. While this family, therefore, shows many illustrations of epigamy, it is one in which the most remarkable illustration of branching in an adult annelid occurs, viz. in *Syllis ramosa* of the "Challenger."

In the Nereidae the epitokous condition is a distinctive feature, resulting in the transformation of a *Nereis* into a *Heteronereis*. The external modifications consist in the great development of the eyes, the alteration of the form of the head, and the remarkable changes in the feet, which are complicated by lamellae and foliaceous cirri as well as by modifications of the bristles. The anterior region of the body remains more or less unaltered, but, in the transformed parts behind, the sexual products accumulate. As Perrier aptly says, it is a caterpillar in front, a butterfly behind. Moreover, the males are different from the females.

Some of the finest Nereids, such as the splendidly tinted *Alitta virens* of Sars, are epitokous forms, and the complexity of the changes in such species as *Nereis dumerilii* are full of interest.

No example is yet known in the Lumbrinereidae, but in the Eunicidae, the well-known *Palolo viridis* of Samoa and Tonga in the

South Pacific affords features closely resembling the epitokous or epigamous condition. Regularly, in October and November the sea around the coral reefs of the islands swarms with fragments of the posterior region of these annelids laden with ova or spermatozoa, but without much change in the bristles. So abundant are they that for centuries the natives have collected them for food.

The Onuphidae, Goniadae, Glyceridae, Opheliidae, Scalibregmidae, and Telethusidae offer, so far as known, no examples of epigamy. In the Chloraemidae the authors hint that *Buskiella abyssorum*, M'Intosh, a form apparently intermediate between the Chloraemidae and the Chaetopteridae, and stretching over a wide area at great depths in the ocean, is an epitokous form, an opinion that is hardly warranted by the facts.

The epitokous condition has not been observed in the Chaetopteridae, or in the Spionidae, though the latter in their young stages are amongst the most conspicuous features of the pelagic fauna. It is likewise absent from the Ariciidae.

In the Cirratulidae the epitokous condition is present in *Dodecaceria*, and it may be in others, such as *Chaetozone*, while schizogenesis occurs in *Ctenodrilus*.[1] In the Halelminthidae, Eisig has shown the detachment of the posterior region with the generative products, but without change of the bristles. Nothing of the kind is known in the Maldanidae, Ammocharidae, Hermellidae, Amphictenidae, Ampharetidae, Terebellidae, Sabellidae or Eriographididae, but in the Serpulidae we have schizogenesis.

The epitokous condition thus characterises Polychaets of considerable activity, the only instance in which it is present in a boring or in a fixed tube-making form being in the Cirratulidae, for the Nereids and Eunicids, though they construct tubes, are comparatively active.

The form which the authors have added to the list of the epitokous annelids was originally described by Oersted, and is not uncommon in the northern seas, though our experience of it in Britain is that it is more abundant in the south.

The authors find three well-marked phases in the life-history of *Dodecaceria*, viz. (1) the sedentary atokous, or ordinary form (A); (2) the large epitokous sedentary form (C); and (3) the epitokous pelagic stage (B).

The first-mentioned form (A) is dark greenish-brown, slightly tapered anteriorly and more distinctly posteriorly. The first region includes the prostomium, and the following segments to the 5th or 7th. The prostomium is devoid of appendages, and presents only a pair of ciliated pits. The next segment (the first metastomial of the authors) bears the grooved palps and two branchial cirri. The succeeding segments (generally 5) have capillary bristles, some of those in the ventral division having a tendency to be spoon-shaped. The authors term this region the thoracic, and it bears the branchiae to the number of 5 or 6 pairs. The rest of the body is nearly uniform in

[1] The position of this form is that assigned to it by the French authors.

structure, but the authors incline to subdivide it into two, from slight differences in the bristles; the second from the 15th to the 25th segment. Each segment bears capillary bristles dorsally, and spoon-shaped ones inferiorly. The latter they compare with a similar type in *Polydora* and *Ephesia*. These bristles in the middle or abdominal region are of service to the animal in its movements in the tube, and consequently undergo certain changes from friction.

The authors found a second and large form (C) somewhat resembling the foregoing in April, but rarely, viz. in the proportion of 1 to 100 of the first-mentioned. It measured about an inch and a half. The segments in the middle region are long, while those anteriorly and posteriorly are very short. It is of a pale colour, with two orange-red eyes. The palps are fully developed. Capillary bristles occur in the first 6 segments. From the 7th to the 26th the dorsal fascicle is composed of long pelagic bristles; from the 27th to the 30th of pelagic bristles and spoon-shaped forms; while in the terminal segments it agrees with the previous form. Ventrally the spoon-shaped bristles also correspond with the latter.

In this form a change, chiefly in the pelagic condition of the bristles,[1] the elongation, and less pigmented condition of the median segments, has occurred. The animal is comparatively sluggish, and secretes abundant mucus, in which the eggs may be placed, though none were seen in it. The genital products fill the body-cavity, the ova (for all were females) being yellowish-brown. The segmental organs open between the feet as in form A—at a higher level than those of B. The wall of the digestive tube is atrophied, but to a less extent than in the latter, and the animal takes food.

The third form (B) is the epitokous pelagic one, which differs very considerably from the foregoing, and also from the first form. It was originally noticed by Verrill in the Bay of Fundy, and specific distinction given it—as *Heteroeirrus fimbriatus*. This second variety, also rare, is recognised by its great activity, for on escaping from the *Lithothamnion* it swims rapidly through the water like *Nephthys*. It has a more slender form. The central region is enlarged by the genital products, and cylindrical. The posterior region is comparatively small, with a slightly spatulate extremity. The muscles of the body-wall are feeble. The anterior and posterior regions are greenish-brown, the middle bright yellow. The eggs are brownish. The head differs from the typical form (first-mentioned) by the presence of two large eyes, in addition to the ciliated furrows. The anterior region agrees in regard to bristles with the first form, except that they are somewhat longer. The palps, however, are reduced by absorption to mere buds, and the branchiae are short. In the anterior part of the abdomen the spatulate bristles have disappeared, and the greater part of the abdominal region is

[1] The elongation of the setae of the antennae of the ostracod, *Philomedes*, at the period of general maturity, is a parallel instance.

furnished with long swimming bristles, like a pelagic *Autolytus*. These take the place of the bristles described in the first form. Some of the spatulate forms remain, however, posteriorly. The perivisceral cavity is frequented by the gregarine *Gonospora longissima*. This form, therefore, is modified for a more or less pelagic existence.

The authors give an elaborate description, with tables, of the setigerous apparatus and the various modifications it undergoes in the several forms (simple and spatulate). They also append a table of the bristles in the developing forms—from those having one setigerous segment to those with eighteen.

The eggs in the first form (A) are bluish-green, and are set free, as usual, from the ovigerous masses into the body-cavity, from May to the end of summer. Moreover, the authors found trochospheres and embryos of several segments in the coelomic cavity—showing that the form is viviparous. When the embryos are large, their number is few in contrast with the eggs, so they suppose that the larvae are extruded by the segmental organs in successive sets. The authors think that the shedding of the reproductive elements is the end of the individual. They also describe, however, brownish masses about the size of these larvae, which may be degenerating forms in process of removal by phagocytes. Further, the authors consider the form is parthenogenetic, for they have found no trace of sperms or of spermatogenesis. Out of several thousands they observed not a single male. The possibility of impregnation from without is not, however, seriously discussed by the authors. They challenge the opinion of Monticelli, who states that this form is a hermaphrodite protandrous one.

In the second form (C) only a single female approaching maturity was observed in April. The ova are smaller than in A and B, and pass out by the segmental organs probably into mucus, unless this form also is parthenogenetic.

In the third form (B) the sexes were found more evenly balanced, and maturity appeared to be in August. In these the eggs were brownish, and larger than in either of the foregoing. In their pelagic condition the ova and sperms are discharged, and development takes place in the sea, the young forms only returning to the alga when their pelagic life ceases.

So minutely did the authors carry out their investigations that they followed the developing forms of the three series in regard to the modification of their bristles and other parts. Thus, in form A, two eyes appeared in the earlier stages, viz. at 15 segments, and disappeared about the stage with 30 segments. The modifications of the short serrated bristles into the spatulate serrated and the spatulate, the appearance of the palps and branchiae, and the growth of the thoracic and the two parts of the abdominal region of the body are also described as far as the truly adult condition.

Of the second form (C), very few of which, however, were procured,

the authors found no young stages, at least none shorter than 57 segments. They noticed the gradual growth of the eyes, the condition of the mucous glands, and the ova. The spatulate bristles agree with those of the first form (A) in having no process (crochet) at the base of the terminal excavation.

In the third form (B) they had 13 specimens, ranging from 19 to 51 bristled segments. No eyes were present in that with 19 bristled segments, and only a pair of palps and branchiae. In the middle of the body the spatulate bristles were pectinated, whereas posteriorly they had the characteristic tooth below the excavation. They found large eyes in one with 45 segments, and the transformation of the bristles was complete at 46 segments (setigerous). The first 36 pairs of dorsal fascicles showed only capillary pelagic bristles. Those with 51 setigerous segments had the palps reduced to buds. The presence of gregarines in the perivisceral space was diagnostic. It is clear that the bristles of this third form arise by gradual transformation from bristles resembling those of the first (A). B gradually undergoes, therefore, a physiological and a morphological transformation, the well-formed eyes, pelagic bristles, and the atrophy of the palps being associated with the development of the reproductive products. The authors, indeed, divide the process into two stages, a first or preparatory period, and a second or period of transformation. Besides the changes already noted in the second stage, the branchiae show signs of reduction and are friable. The animal ceases to take food, and its digestive canal atrophies (walls become thin), the reserve eosinophile granulations are used up in the development of the reproductive elements in the body-cavity, and segmental organs are developed in all the abdominal segments.

In reviewing their observations on the three forms and drawing conclusions, Caullery and Mesnil consider that the three forms pertain to the same polymorphic species, yet no males have been seen either in the first or second form. Moreover, the latter (C) has been found only in the adult and larger condition, and may be the final condition of the former (A).

The authors review the groups of the Cirratulidae, which they divide into those with and those without large palps. They then descant on the genus *Heterocirrus*, particularly on *H. viridis*, Langerhans, a species having capillary bristles and long hooks, and which assumes an epitokous condition—distinguished by the long swimming bristles, the atrophy of the digestive tube, and the enlarged eyes. A similar condition is met with in *Heterocirrus caput-esocis*, St. Joseph. They also allude to the fixed epitokous condition of *Thoryx marioni*, recognised by the absence of eyes and the long capillary bristles. *Choetozone*, again, is a genus which, throughout all its stages, has the facies of an epitokous form. They also institute a comparison between *Ctenodrilus* and *Dodecaceria*, showing that their external form and internal structure have considerable resemblances. The authors, in fact, consider

*Ctenodrilus* an arrested representative of the Cirratulidae, such a condition being in agreement with the parasitic life led by some in the coelomic cavity of Holothurians and Synaptae. The gemmiparous condition of *Ctenodrilus* is probably associated with its parasitism.

They consider that schizogamy is derived from epigamy, as Ed. Meyer, Kleinenberg, and Eisig do. On the other hand, Ed. Perrier regards schizogamy as the primitive condition, interpreting, for instance, *Heteronereis* as a colony of two individuals, the metamorphosed region presenting in an aborted condition what is more fully seen in the primitive state in the schizogamous Syllidians. They observe that an annelid normally increases by new segments in front of the pygidium, and they regard as a complication the appearance of a formative region at any other point. In other words, schizogamy is distinguished from epigamy by that addition only, and logically, therefore, the former is derived from the latter.

In some forms epigamy, or the epitokous condition (*Heteroeirrus* and *Mystides*), is very simple, viz. the addition of tufts of capillary bristles in a certain number of feet—a primitive modification resulting from a physiological condition, and rendering the animal more mobile when discharging its reproductive elements.

Greater complication is seen in *Exogone* and *Heteronereis*, yet only one region of the body is much modified. Even in *Mystides*, *Heteroeirrus*, and *Dodecaceria* (B), the anterior segments do not acquire the pelagic bristles, which occur only in the region behind; and this agrees with the functions of the parts, the anterior directing the movements of the succeeding propulsive region.

In some forms, at sexual maturity, as in *Notomastus lineatus*, Eisig has shown that the abdominal region, charged with the genital products, separates from the thoracic for a pelagic life, yet no morphological change has taken place. A similar condition occurs in *Palolo viridis*, the edible annelid of the Samoans and Tongese.

The authors suggest as a plausible hypothesis that the phenomena of schizogenesis progressively follow schizogamy, by an accelerated embryogeny; as, for instance, in the Syllidians. The consequence of this acceleration is to make the propagation of the species doubly sure, and independent of ordinary sexual reproduction.

The internal modifications of such forms at the epoch of sexual maturity are the rapid absorption of the stored materials, *e.g.* the eosinophile granulations, the atrophy of the walls of the digestive canal, the diminution of the muscles of the body-wall and its granular ectodermic layer, and the prominence of the segmental organs.

They group the phenomena thus apparent into three divisions, viz.:—

1. Phenomena of histolysis without external metamorphosis or scissiparity . . . . { *Glycera capitata.* / Phyllodocidae. / *Polyophthalmus.*

2. Phenomena of histolysis without external metamorphosis, but with scissiparity . . . . { *Clistomastus lineatus.* *Palolo viridis.*
3. Phenomena of histolysis, accompanied by external metamorphosis (epigamy or epitokous condition), and sometimes by scissiparity (schizogamy) . . . . . { *Heteronereis.* Cirratulidae. Syllidae.

They point out the comparative frequence of the epitokous condition in the annelids, and, moreover, that the phenomena of histolysis associated with sexual maturity is very general in the group. Thus there is a close parallelism between them and certain fishes, for instance the salmon, as ably shown by Miescher of Basle, and Noël Paton of Edinburgh.

The fourth chapter of the paper is devoted to a consideration of the various adaptations of annelids for a pelagic life—in order to ascertain what connection the temporary pelagic condition at sexual maturity has with a completely pelagic life. Pelagic annelids may be grouped (1) into those only pelagic at sexual maturity, (2) pelagic larval forms, (3) those permanently pelagic.

Epitokous annelids fall under the first category, and in some their existence is brief, as, for instance, in the case of *Palolo*, which gives an example of pelagic agony rather than a pelagic existence. On the other hand, *Exogone* survives the period of reproduction, and sheds its pelagic bristles.

Most annelids, whether errant or sedentary, have pelagic larvae—generally with long pelagic bristles—and the authors quote Haecker to the effect that these long bristles are not only organs for locomotion, but aid the animal in hovering and other movements. There can also be little doubt that these bristles are—in such as the larval Spionidae—protective, and are used by the larvae very much as the Polynoidae use their bristles when attacked.

The third group, viz., permanently pelagic annelids, is characterised by the transparency of their tissues, large eyes, delicacy and length of bristles. Certain Phyllodocidae, the whole of the Alciopidae, the Typhloscolecidae, and the Tomopteridae (devoid of ordinary bristles) are examples.

The authors conclude that it must at present be admitted that adaptation for a pelagic life may take place independently of the epitokous condition. Indeed, it would have been well if the authors had attended to cases, *e.g.*, the Leeches and Nemerteans, in which active pelagic habits can be assumed without a trace of bristles, and in which the habits are not associated with reproduction, and not always with a permanently pelagic condition. It is the muscular system rather than the bristles which is mainly concerned, and it does not follow that long delicate bristles are better fitted for swimming than for hovering in the water.

The last short chapter contains remarks on viviparity amongst

the Polychaets. The authors cite the peculiar cases of parasitism of one annelid in another, as *Lumbriconereis* in *Marphysa*, one of the Eunicidae. Krohn, however, found a viviparous *Syllis* at Nice, in which the embryos in the body-cavity occupy a third or a fourth of the length posteriorly. Levinsen describes briefly a similar instance in *Syllis incisa*. Claparède and Metchnikoff found a viviparous Cirratulian at Naples, the young having two eyes, two pairs of branchiae, and similar touches of pigment to the adult. The authors, as already mentioned, indicate that their form A (*Dodecaceria*) is viviparous, but further observations are needed. Viviparity, again, occurs in *Nereis diversicolor* and *N. dumerilii*. The former, a protandrous hermaphrodite, was found by M. Schultze to produce reddish-brown ciliated pyriform larvae furnished with a mouth and incomplete intestine. The segmentation of the eggs has been seen by Schroder; while Mendthal thinks they may only be exceptionally developed in the interior of the mother. The authors suggest that it may be a case of self-fertilisation. The viviparity of *Nereis dumerilii* was observed by Metchnikoff in a hermaphrodite form, yet though the authors found many at La Hague, no trace of the condition was seen, so that this feature may be only occasional, as in the Serpulidae. They also incline to the opinion that in *Dodecaceria concharum* (A), in *Syllis vivipara*, and *S. incisa*, in *Cirratulus chrysoderma*, the forms are unisexual, and that, therefore, parthenogenesis occurs. As formerly stated, however, further observations are required before certainty is reached.

It will thus be seen that while the able authors have increased our knowledge considerably in regard to *Dodecaceria*, yet even in this form several important features remain for future investigators, such as the exact relationship of the three forms A, B, and C, and the development of each. These problems rest with southern observers, since *Dodecaceria* is rather rare in our colder northern waters.

The epigamous condition in annelids has, perhaps, its analogues in the medusoids of the Hydrozoa, the strobila of the tape-worm, the imago and specially the queens of certain insects, and the changes in the male cephalopod.

The relation of the changes in certain vertebrates is not so clear, but yet there is a thread of analogy in the alterations of the mandible of the male salmon, the coloration of the lump-sucker and other fishes at the breeding season, the changes in the air-sacs of certain amphibians, in the plumage of birds, in the passage of the testes outwards in certain mammals at puberty, and the development of fat, hair, and horns on certain regions.

The free discharge of the ova from the ovaries of the higher forms, their passage along the oviduct to the uterus, the development of the embryo in this organ, and lastly its extrusion externally, are only

further changes brought about by the high degreee of specialization in the mammals, and by the fact that the procreative power is continuous for a definite period after puberty. In the case of the annelid the changes in the individual are evanescent or terminal, whereas in the higher forms they are more or less permanent.

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# Mehnert's Principles of Development.

By Professor J. Arthur Thomson, M.A.

It is well known that the rapid development of embryology has been mainly along morphological lines; of the physiology of development we know extremely little. For a time the gap was filled by generalities—laws of growth, principles of development, polarities, and the like, in fact a plethora of vague speculation. Then came a stern reaction, a franker confession of ignorance, and a renewed enthusiasm for concrete research, as illustrated for instance in the recent boom of experimental embryology. Mehnert's work[1] is not experimental, but it abounds in measurements, and it is this that forms its attraction, for the beginning of measuring is often the beginning of science.

Two years ago Dr. Ernst Mehnert published an essay entitled "Kainogenesis," in which he gave the results of a comparative study of 384 extremities of various embryos from turtle to ostrich, and formulated certain general embryological conclusions. Far from accepting the prevalent vague idea of uniformity in development, he showed that each type or even species has its individuality as regards the order in which various parts appear and the rate at which they develop. He gave many illustrations proving that the time at which a structure appears in the individual development cannot be taken as indicative of the historical or phylogenetic age of that structure. In this regard he showed clearly that the familiar generalisation "ontogeny is a short recapitulation of phylogeny" must not be taken too literally. Not that our author doubts the famous biogenetic law; he only points out that in applying it care must be taken to appreciate the effects of varying rates of development in different creatures. It is this notion of time-displacements in development which has been elaborated in the second essay on the principle of organogenesis. In other words, the author seeks to show the importance of the developmental changes which may result from changes in the growth-rate of particular parts.

[1] "Biomechanik erschlossen aus dem Principe der Organogenese." By Dr. Ernst Mehnert. 8vo, pp. viii. + 177, with 21 figs. Jena: Gustav Fischer, 1898. Price 5 Marks.

Beginning with the vascular system, which he regards as essentially entoblastic in origin, and as originally derivable from a dorsal aorta or pair of arteries and a sub-intestinal vein, connected by metameric cross-vessels as in some Annelids, Mehnert maintains that the heart is a secondary differentiation, arising as the result of functional adaptation from a portion of the primary ventral vessel where the greatest resistance had to be overcome. But when we turn to the actual development of the heart, we find that in lamprey and dogfish, newt and frog, chick and pig, rabbit and man, the heart is differentiated *before* the associated main vessels. There is thus an obvious lack of congruence between the ontogenetic or individual, and the phylogenetic or racial, order of sequence,—a discrepancy which the author deals with in his proposed corollary to the biogenetic law. This corollary is, that as an organ rises in physiological importance and structural differentiation, it acquires a proportionate increase in its rate of development. In fact, as the author expresses it, the embryological key to phylogenetic age is not to be found in the order of appearance, but in the intensity of developmental energy. But what this developmental energy precisely is we are not told. A second illustration concerns the mammary glands. It is generally supposed that these have arisen from more primitive integumentary glands of less specialised type, such as the sebaceous or the sudorific glands. The debated question which of these is the more nearly related to the milk glands need not concern us now. What is striking is the fact that the primordia of the mammary glands appear in man, for instance, before there is any hint of sudorific or sebaceous glands. Again the individual development seems to reverse that of the race; again Mehnert gives the interpretation that the acceleration of the mammary glands is causally connected with their progressive physiological importance and structural specialization.

A third, and, as it seems to us, much less effective illustration is found in the pineal eye, which Mehnert regards as historically older than the paired eyes and possibly homologous with the eye of Tunicates. But in all vertebrates hitherto studied embryologically the paired eyes, if present, arise decidedly earlier than the pineal eye. The organ which has lost its functional dignity, so to speak, is handicapped in its rate of development.

Communications between the cavity of the "protovertebrae" and the cavity of the lateral plates are primitive in lower vertebrates like the lancelet and selachians, but they are not seen in the early stages of bird and mammal, yet Bonnet has described these communications in four segments in a *late* embryo of the sheep (15-17 days), and Dexter has made a similar observation in regard to the chick. This late occurrence of a structural detail which is doubtless on the retrograde path harmonises with Mehnert's view. Retrogression and retardation go hand in hand. The late occurrence of a neurenteric

canal in the higher vertebrates, and the late occurrence of cartilaginous connections between pieces of skeleton which arise isolated from one another, *e.g.* the connection which Wiedersheim demonstrated between the head of the femur and the pelvis in amphibians, are other cases in point.

After discussing the above instances in detail the author gives a tabular summary, the general result of which is, that phylogenetically progressive organs exhibit a general acceleration in their rate of development, while retrogressing organs tend to be more and more belated. A structure whose racial importance is waning cannot be *simply* suppressed; a structure whose racial importance is increasing cannot be *simply* exaggerated; there is no question of *per se* lopping off or tacking on; the embryological facts point to "an alteration of the entire course of development from the first appearance of the primordium (rudiment, *Anlage*) to the cessation of the process." This conception may perhaps be harmonised with Weismann's theory of a germinal struggle between the waxing and the waning determinants (or let us say structural units), — an unseen struggle within the germ-cell whereby an intra-selection abets the ordinary process of natural selection. The determinants which are in force in the germ, which secure success in nutrition and otherwise, will have a free course and an accelerated development. Having illustrated his central thesis,—that the rapidity of the growth of an organ is proportional to its degree of development, Mehnert proceeds to further analysis.

He first defines abbreviation as a premature cessation of development, illustrated for instance by numerous skeletal parts which do not complete their natural histological differentiation,—a cartilaginous metastyloid in a man fifty years of age, a cartilaginous epipubis in an ostrich twenty-five years old, or the more or less persistently cartilaginous sternum of certain cetaceans. From this "abbreviation" he distinguishes "retardation" or the slowing of the developmental process, as in the case of a wisdom tooth. Abbreviation is the insertion of a full stop at an early stage; retardation is slow growth long drawn out. But the two modes of retrogression may be combined.

Similarly, in regard to progressive structures, the author defines acceleration or the quickening of developmental processes, which he illustrates chiefly by comparing the different rates of growth of various skeletal parts during foetal life; and prolongation or the lengthening of progressive development, which he illustrates by the earlier and earlier appearance of certain primordia (*e.g.* of metacarpals and metatarsals), and by the extension of the growing period without loss in rate. But, again, the two modes of progression may act in concert.

In the turtle the long bones, such as humerus, femur, radius, and ulna, arise from primordia which differ but slightly from the average size of a carpal or phalanx rudiment. But although they start very

much alike, they do not grow at the same rate, and Mehnert's conclusion, which he seeks to establish by statistics, is that the rapidity of growth is proportional to the degree of development finally attained. The long femur grows most quickly, the short first phalanx of the thumb is slowest of all. The rapidity of the ontogenetic development (growth-process) of an organ is proportional to the height of differentiation which is eventually reached. And the rapidity is believed to be on the increase from generation to generation in the case of all structures which are still evolving.

This acceleration is said to be due to a cumulative inheritance of developmental energy. But we cannot rest here. It must surely admit of further analysis in terms of the conditions favouring cell division, the production of nuclein substances, and so forth.

Here then we have the author's thesis which claims rank as an induction, and it becomes of interest to hear what he has to say on the general problems of embryology. Even if we do not accept his conclusions, it is instructive to have the vexed questions re-discussed from a fresh standpoint. The endless references and the ten pages of bibliography show that the author has not been partial in his survey.

Like His, who first pointed out thirty years ago (1868) that growth proceeded at different rates in different parts of the embryo, and that the rate of growth increases with the physiological dignity of the structure, Mehnert believes that the conception of organ-forming germinal areas ("organbildende Keimesbezirke") is inevitable. He supports this by reference to the numerous independent individual variations in rate exhibited in the development of organs, which suggest that each part pursues its own path, now hastening and again lagging, and also by reference to Born's wonderful experiments on grafting tadpoles, where organs separated from their natural neighbourhood went on developing in a manner certainly suggestive of "self-differentiation." At the same time, Mehnert does not go to the extreme of denying that the development of an organ is influenced by its environment in the widest sense, *e.g.* by the pressure of an adjacent organ or by gravity.

In a number of curves and tables, the author gives the results of his measurements of the rate of growth from the beginning of the rudiment of an organ onwards. These all relate to parts of the skeleton, and show (in man) a rapidly ascending curve on to birth, and then a rapid decrease followed in early childhood by a slow rise. It seems to us that the exposition of these measurements, on which the whole point of the book depends, is very inadequate, the curves are badly done and most tedious to decipher.

He ventures to distinguish the first apex of the curve on to birth as the climax of the preformed character ("Evolutionselevation"), and the second in early childhood as the expression of acquired character ("epigenetische Elevation"), or the result of "functional hypertrophy"

induced by new relations to the external conditions of life. In other words, he regards the growth of an organ as the result of two factors. One of these consists of the inherited qualities,—the constitutional energies. The other (epigenetic) factor is in a word environmental.

The primary, constitutional, or inherited factors (Evolutionsdetermination) are fixed when the germ-cell is separated from the parent which bears it, or when fertilisation has occurred. The secondary, functional, or acquired factors (Entwickelungsdetermination) are found in the environmental conditions and in the internal relations of parts. Many have made this theoretical distinction before, the difficulty is to apply it.

It is easy to show that the course of development may be modified by external circumstances and bythe immediate organic environment, and the author recalls many more or less familiar instances of determination by nutrition, by temperature, by actual functioning, by association with other parts, and by the environment in the broadest sense. But it seems to us that the conception of environmental relation requires more careful analysis than the author gives it; thus we have to distinguish (*a*) the relation of normal functional dependence, (*b*) the relation which results in a "modification" in the strict sense, *i.e.* when the limit of organic elasticity is transcended, and (*c*) the relation in which the environment supplies a variation-stimulus or inhibits a variation-tendency.

In regard to heredity, it seems to Mehnert that if a correlation be admitted between the rate of embryonic development and the functional importance of an organ, we are forced to admit the inheritance of acquired characters, to doubt which seems to him almost a reproach to the intelligence of a physician or observant naturalist! He insists on the parallelism between the racial evolution of an organ and its growth in the development of the individual, and seems to suggest that the former explains the latter.

But the peculiarity, the idiosyncrasy, of Mehnert's view is, that there is a transmission of specific energies from the parent to the germ-plasm which is the foundation of the offspring. The body of the parent is to the germ-cell as a mother-magnet to a daughter-magnet. There is in inheritance, he conceives, a constant element, the material germ-plasm; but there is also a variable factor, namely, this mysterious energy on the intensity of which the rate of development depends. In this hypothesis he does not mean to postulate an imponderable vital force; he heads his work with Ostwald's axiom that everything is to be interpreted in terms of energy.

If each step in racial evolution means not a mere lopping off or tacking on, but an alteration in the developmental rhythm of the organism, which seems a most reasonable conception, then the talk one sometimes overhears about uniformity of development had better be silenced, and already many only whisper the old error. Mehnert shows

his learning and experience to good account when he argues that in each species there is an individuality—eine Specificität—in development. And since there is variability each development is literally a "Kainogenesis." This is one of the strongest sections in the book. Mehnert goes on to point out that an acquired character such as the strength of an athlete's biceps has to be sustained by a continual repetition of the original stimuli which produced it, whereas congenital characters are at least represented, though not fully developed, even when they do not receive their appropriate functional stimulus. Persistence and length of life depend primarily on the constitution, there is only indirect gain in the acquired. It may save the life of the individual in struggle; it may, according to the modern doctrine of indirect selection or "organic selection," save the race until a constitutional variation in the same direction is evolved. But to this consideration Mehnert adds his particular theory of cumulative energy. The life of a species runs on a principle of compound interest; there is a cumulative augmentation of the mysterious specific developmental energy.

The author makes a brave attempt to keep faith with the recapitulation doctrine, and with himself. He admits the individuality of development; he admits that the order of development in ontogeny is often the reverse of the order of evolution in phylogeny; he admits that we have to look for a recapitulation of stages in organogenesis rather than of stages of entire organisms, as Dr. Beard has also maintained; but he clings fondly to the old conclusion, provided it be corrected by the notion of a variable rate of growth. There is a suggestion here of the old shoe cobbled so often that there is nothing of the original left.

In the section on the degeneration or involution of organs, Mehnert points out that this is, so to speak, normal. The organism is an integrate of parts which have different rates of development. It is like the march of a great procession; some members are continually falling behind, others are always shooting on ahead. Involution and evolution go hand in hand. The ovum develops, the polar bodies degenerate; the blastoderm develops, the merocytes degenerate; the head grows and the tail dwindles; the higher nerve-centres of the Mammalian brain increase and the occipital region diminishes; the metanephros comes and the pronephros goes; the backbone strengthens and the notochord disappears; and so on throughout, a continual necrobiosis.

Mehnert's essay might be in part described as a recognition of the time-element in development. The material of evolution is slow to change in kind—it is the germ-plasm—but its rate, and all matter has its rate, is more readily altered. We may not transmute the metals, but we can turn gas into fluid or solid at will, *i.e.* we can change the rate of molecular movement.

Thus our author ends fitly by expounding the effect of altering the

date of sexual maturity. Some ctenophore larvae are mature almost immediately after they leave the egg, but this is not true of the eagle. And though there are disadvantages, there are also some advantages on the side of the eagles. There is opportunity for the body to influence the germ-plasm, which nobody can deny. But having said so much we need not follow the author into his discussion of premature sexuality or neotaenia on the one hand, or of the advantages of late marriages on the other.

It must be confessed that the book we have been dealing with is hard to read and harder to expound, and it is quite possible that we have in various instances failed to catch the author's meaning. It is not easy to estimate how much it really amounts to—this "principle of organogenesis." But as our position in regard to the physiology of development may be compared to that of travellers hesitating in thick mist by the edge of a bog where path and pitfall are alike hidden, it would seem that he must be either a very wise or a very foolish person who takes it upon himself to dismiss lightly the suggestions of a measurer.

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# A Note on Telegony, Xenia and "Hybrid Oology."

By G. P. Bulman.

A good sub-title for this paper would be "not proven"; as its object is to point out that though we have few grounds for any belief in the above phenomena, we have still fewer (apart from our personal wishes regarding our view of the causes of heredity!) for disbelief.

## *Telegony.*

The phenomenon for which this word was suggested [1] is, as telegony, completely hypothetical at the present moment, and by a large number of scientific men is regarded simply as a manifestation of reversion. However, some consider that although in the great majority of cases the effect is probably due to reversion, yet there are a few cases (perhaps even less than one or two per cent as mentioned by Romanes [2]) in which an effect is produced which is telegonic. The fact that all those cases which are known, can be as easily explained by the one as by the other, makes scientific men feel rather shy of introducing a new cause. A good catalogue of such cases will be found in *Natural Science*, Dec. 1893, pp. 436-440, by Mr. F. Finn.

The first duty, therefore, of a student of this problem of heredity is to prove its existence, and I think the best way to do this is to reverse the usual experiments, and see if the progeny of a less highly specialised pair do not sometimes display traces of the previous union of the mother with a more highly specialised male; these effects could not be due to reversion.

My friend, Mr. Alex. Meek, and I are at present engaged in conducting experiments of this nature; but telegony is probably of such great rarity that it would be necessary for a very large number of verdicts of "not proven" to be given before any conclusion as to its non-existence could be arrived at. Perhaps the best experiment in this way is that of the rabbits suggested in our letter in *Natural Science*, Feb. 1899, p. 166.

[1] A. Weismann, "The Germ-Plasm," p. 383.

[2] G. J. Romanes, "An Examination of Weismannism," p. 199.

Should we consider that there is sufficient reason for supposing that telegony does sometimes occur, and is an occasional factor in heredity, we must then turn our attention to its cause, as it might prove of the greatest importance to any theory explanatory of this subject.

I think the explanation of the phenomenon given by Romanes [1] is the most satisfactory, for that given by the supporters of Pangenesis, and other like theories, postulates such an extraordinary activity and plasticity on the part of the spermatozoa and their contained gemmules. After reaching the ovaries too late for the fertilisation of the mature ovum they must be supposed to penetrate the walls of the generative organs and fuse with the somatic cells. Then these cells must cast off gemmules which by some means or other must reach and form part of the succeeding and yet immature ova.

The provisional explanation suggested by Weismann,—that the spermatozoa might penetrate the succeeding ova while yet immature, has two objections to it,—first, the improbability of the efficiency of the spermatozoon to penetrate the follicular coverings of an immature ovum; and secondly, the objection mentioned by Weismann himself, that "a supplementary fertilisation of an egg-cell in this manner must be considered possible. . . . But this has never been known to occur." [2] The two explanations mentioned by Darwin [3] may be passed over with the barest mention, the one of the intercommunicating blood vessels between the embryo and mother, because it does not hold good in the case of birds, and from Mr. W. Heape's splendid experiments [4] does not affect mammals; and that of imagination because "there are very slight grounds for any such belief." [5]

As far as I know, the suggestion of Romanes has never been discussed since. It appears the simplest theory, and is one which, instead of showing telegony in the light of an objection to the Weismannian (or, in fact, any other) theory, is in perfect harmony with it.

## *Xenia.*

This, the direct action of the male element on the mother-plant, is an extremely hypothetical theory, the only facts on which it is based being those mentioned by Darwin.[6] De Vries [7] and Focke [8] have both expresssed their doubts as to its occurrence, and until fresh experi-

[1] Viz. "that the life of 'ids' is not commensurate with that of their containing spermatozoa. . . . And . . . when subsequent ova mature . . . these dormant ids adhere to their porous walls, through which they may pass."—*Op. cit.* p. 198.

[2] A. Weismann, "The Germ-Plasm," p. 386.

[3] C. Darwin, "Variation under Domestication," vol. i. p. 437.

[4] See *Nature*, December 30, 1897, in which is a report of Mr. Heape's paper (published by the Royal Society) on the "Transplantation and growth of mammalian ova in a uterine foster-mother."

[5] C. Darwin, "Variation under Domestication," vol. i. p. 437.

[6] *Op. cit.* p. 427 *et seq.*

[7] H. de Vries, "Intracellulare Pangenesis," p. 206.

[8] Focke, "Die Pflanzen-Mischlinge," p. 500 *et seq.*

ments, which are urgently required, establish it beyond doubt, I think Weismann is fully justified in disregarding it.

### "*Hybrid Oology.*"

By this term (a very clumsy one, I confess—but *faute de mieux!*) I wish to denote that extraordinary phenomenon, which has been so often reported in connection with the eggs laid by a bird when fertilised by the cock of a different species. I refer to the supposition that at the time of fertilisation, not only is the ovum fertilised, but that part of the oviduct which secretes the shell is affected in such a manner, that the ovum on passing through is provided with a shell which partakes of the tints of the eggs laid by the cock's own species—and not only the tint but also the microscopic structure.[1] Instances of this kind are mentioned by Dr. Hans Gadow in Bronn's *Thierreich*, vol. Aves, p. 874, and by Nathusius (*Journal f. Ornithologie*, 1874, p. 1), and a few have come to my own knowledge. A friend has informed me of a case in which he kept some fowls of the Orpington variety separate for a time, when they laid eggs of a buff (the ordinary) tint. He then allowed them to run loose in a large yard, in which were fowls of all sorts; but after a few months he again confined them in a separate pen. But for several weeks after the eggs laid by them were pure white. The Orpington breed having been so lately developed, I should have thought that they were simply reverting to ancestors of a white-egg-laying variety, had I not heard at the same time of two similar cases regarding black Spanish, a comparatively old breed. Mr. Meek tells me that in the magnificent collection of Mr. W. Mark Pybus, Newcastle-on-Tyne, he saw a clutch of eggs which had been laid by a black Spanish hen when fertilised by the black cock. While the eggs showed both in form and texture the well-known properties of those laid by the hen's species, in colouring they were almost indistinguishable from the cock's.

In the case of "hybrid oology," as in that of xenia, experiments have not been conducted to any great extent, and in all probability the supposed effects have arisen from some other cause, such as a previous cross.

I do not think we ought to rest until these three hypotheses are either proved to be justifiable, or, after an overwhelming number of unsuccessful experiments, relegated to the land of myths.

[1] A. Newton, "A Dictionary of Birds," Part I. p. 190.

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# FRESH FACTS.

Partial and Total Conjugation. Hans Wallengren. "Ueber die totale Konjugation bei Vorticellina," *Biol. Centralbl.* xix. 1899, pp. 153-161, 3 figs. In *Epistylis simulans* and other vorticellids the process of "total conjugation" is not strictly total; the nuclei and most of the endoplasmic material of the microgonidium pass into the macrogonidium, but a shrivelled residue is left,—not quite dead, but fatally injured. So that the difference between total and partial conjugation is one of degree.

The Crop of Birds. Gust. Swenander. "Beiträge zur Kenntnis des Kropfes der Vögel," *Zool. Anzeig.* xxii. 1899, pp. 140-142, 4 figs. In this preliminary communication the author distinguishes four types of crop,—of Pigeons, of Raptores, of Rasores, and of Fringillidae and some aquatic birds.

Chambered Stomach in a Monkey. A. Pilliet and R. Boulart. "Note sur l'estomac composé du Semnopithèque," *Comptes Rendus Soc. Biol.*, Paris, v. 1898, pp. 216, 217. The authors have elaborated Owen's account of the complex stomach of *Semnopithecus nomaeus*, which is so strongly suggestive of that of ruminants. A large cardiac portion, like a paunch, with numerous internal papillae, leads into a median portion with numerous side reservoirs, and beyond this there is a ridged cylindrical pyloric portion with two fibrous bands. The first portion is a storage area, the second portion is a true stomach, the third portion is divided by a groove from the oesophagus into two areas, of which that above the groove is oesophageal in character, while that beneath resembles the second portion. It is an interesting case of convergence.

Comparative Embryology of Thymus. F. Maurer. "Die Schilddrüse, Thymus und andere Schlundspaltenderivate bei der Eidechse," *Morph. Jahrb.* xxvii. 1899, pp. 119-172, 3 pls. 4 figs. In connection with gill-clefts, two sets of organs must be distinguished:—(*a*) Those which appeared in forms in which there was an open and functional gill-cleft system (thyroid, thymus, and post-branchial bodies), and (*b*) those which appeared only after the obliteration of branchial respiration (epithelial corpuscles and carotid gland). The organs of the first set develop in the lizard in a manner essentially the same as in Anamnia; the detailed differences are shown in comparative figures.

Pulmonary Ganglia. A. E. Mettam. "On the presence of ganglia upon the pulmonary nerves" (a preliminary note), *Proc. Scottish Micr. Soc.* ii. 1898, pp. 195-203. The author calls attention to the relative abundance of ganglion cells in connection with the pulmonary nerves in the lungs of certain domesticated animals, *e.g.* sheep, during foetal life; and suggests that they may act as local respiratory centres until the reflexes are fully established.

Temperature and Regeneration. Florence Peebles. "The effect of temperature on the regeneration of Hydra," *Zool. Bulletin*, ii. pp. 1899, 125-128. The author shows that the rate of regeneration in *Hydra grisea* and *H. viridis* varies with the temperature, the optimum being between 30° and 38° C.

POISONS OF PARASITES. G. H. F. NUTTALL. "The poisons given off by parasitic worms in man and animals," *Amer. Nat.* xxxiii. 1899, pp. 247-249. In a short article in the March number of our American analogue, Mr. Nuttall sums up the evidence advanced by Peiper and others to show that a number of parasitic worms, both nematodes and cestodes, give off poisons within the body of their hosts. These poisons may account for some of the peculiar symptoms induced by the presence of the parasites.

SUBMARINE PHOTOGRAPHY. L. BOUTAN. "L'instantané dans la photographie sous-marine," *Arch. Zool. Expér.* vi. 1898, pp. 299-330, 4 pls. 8 figs. The plates accompanying this paper show that the author has attained some measure of success in submarine photography. At a depth of three metres good instantaneous photographs may be obtained by utilising sunlight only and without getting into the water. It is probable that photographs may be effectively taken at much greater depths by utilising a powerful artificial light.

ROCK STRUCTURES. CATHERINE A. RAISIN. "On certain structures formed in the drying of a fluid with particles in suspension," *Proc. Roy. Soc. Lond.* lxiii. 1899, pp. 217-227, 2 pls. The powder of various rocks, when mounted in water for examination under the microscope, exhibits interesting forms when dried. The paper considers the origin and method of experiments, classification of forms produced, conditions and causes of formation, possible applications in nature, and results affected by crystallisation. The results may throw light on the origin of certain minor structures in rocks.

AT the March 21 meeting of the Zoological Society of London, Mr. F. E. Blaauw gave an account of the breeding of the Weka Rail (*Ocydromus australis*) and Snow-Goose (*Chen hyperboreus*) at Gooilust, N. Holland. The Rails could not be induced to complete the periods of incubation, always eating the eggs after sitting for a few days. One young was eventually hatched by placing an egg under a bantam-hen. The Snow-Goose (a female) paired with a male Cassin's Snow-Goose (*C. caerulescens*), and laid and hatched three eggs. The young birds, it is said, were apparently assuming the plumage of the male parent.

# SOME NEW BOOKS.

## "TO COUNT THE SEAS ABUNDANT PROGENY."

The Resources of the Sea as shown in the Scientific Experiments to test the Effects of Trawling and of the Closure of certain Areas off the Scottish Shores. By W. C. M'Intosh, M.D., LL.D., F.R.S., &c. 8vo, pp. xvi. + 248, with illustrations and 32 tables. London: C. J. Clay and Sons (Cambridge University Press), 1899. Price 15s.

"There are as good fish in the sea as ever came out of it" is apparently Professor M'Intosh's matured opinion, and he would probably add—"and as many." He has had great opportunities, given to few, and from his very extended connection with fisheries' investigation he, if any one, is entitled to hold opinions of his own on the matter. So, as what we want on all fishery questions is light—more light, and free discussion, and especially *facts*, we welcome this book even although (not *because*) it may be interpreted by some as an attack upon the methods and conclusions of the Fishery Board for Scotland, with which Professor M'Intosh was at one time connected. We hasten at once to remove any possible false impression that our last statement may give rise to, by saying that we do not for a moment mean to indicate that the author shows any undue hostility to the Board. On the contrary, he seems to us to be at least animated by a fair and judicial intention in his treatment of the Board's statistics and reports. He is, however, severe in his occasional remarks upon the inefficiency of the Board's steamer, the *Garland*, and the general feebleness of her performances compared with the commercial trawlers; and his conclusions as to the state of the fish population round our coast, and as to the effects of trawling and of closure of areas, are diametrically opposed to those given in the Board's recent blue-books.

Professor M'Intosh's assumption of the judicial aspect is so marked in places that one is reminded of the gods sitting up aloft distributing merited censure and approbation with an even hand upon the mere mortals below who have had something to do with fishery matters—upon scientists at biological stations from Plymouth to Japan—upon chairmen of Boards, county gentlemen and sheriffs—the work of two of the English Sea-Fishery Committees even is mentioned with approval. All will concur, however, in the well-deserved tribute of respect which Dr. M'Intosh pays to the memory of Lord Dalhousie, "the popular statesman who was at the head of the Royal Commission (1883-85)"; and it would be well for those engaged in fisheries work always to bear in mind Lord Dalhousie's wise caution, "Remember others will follow, criticise, and check every step taken." We believe that those responsible for the scientific work of the Fishery Board for Scotland will, looking at the matter in this spirit, welcome Professor M'Intosh's criticisms of their methods and conclusions. But we do not for a moment suppose that they will accept his views, or leave his statements unchallenged. The Board, or their able scientific secretary, Dr. T. W. Fulton, will probably have a good deal to say in favour of their own reports,

and be able to give some solid grounds for the faith that is in them—a faith in the (ultimate) beneficial effect of closure against trawling, which has led them to extend their experiment from the Firth of Forth and St. Andrews Bay to the Clyde sea area, and finally to the vast extent of the Moray Firth.

And it is not difficult to see what an effective reply to the present book might consist in. Professor M'Intosh alludes in his preface to the 108 tables compiled from the Board's trawling statistics, and he gives us some thirty of these as supporting his case; but these, and the statements as to the different fish, strike the ordinary scientific reader as being meagre—the numbers dealt with are comparatively small, the occasions are few, and the areas traversed are not extensive. Moreover, Professor M'Intosh makes so much of the unsuitability of the *Garland* for the work, the smallness of her trawl, and the uncertainty of such results (p. 114, etc.), that one rather wonders that he was willing to trust any superstructure of argument and conclusion upon such slender and broken reeds. The deceptive nature of insufficient statistics is proverbial. We do not believe that the *Garland's* statistics are sufficient, as yet, to prove anything—except perhaps what Professor M'Intosh so frequently points out, the uncertainty of all such operations. Our critic shows that the results in the Board's first quinquennial period cannot fairly be compared with those in the second, since they were obtained under different conditions. We doubt whether there is a sufficiently extensive body of figures in any period to compare with any other with the view of arriving at such conclusions as those of the Board on the one hand or those of its critic on the other. It is probably premature to expect any important conclusions yet. It may be thought that ten to fifteen years is a sufficiently lengthy period of observation to justify a belief that results unvitiated by "accidental" and seasonal variations might have been attained. And that would have been the case if we had had before us all the results of the hauls of all the commercial trawlers working round our coasts during that period. [Why do not the Board of Trade in England and the Fishery Board in Scotland devise a mechanism for obtaining these most important returns?] But what can we expect from the occasional isolated scrapes (for they are little more than that) of a steam-yacht with a small trawl? There is one thing—amongst many—that Professor M'Intosh has made clear in this book, and that is, if the Fishery Board continue the experimental closure of sea areas for some years scientific men will expect them to do their best to settle these disputed questions by adopting a more vigorous and effective policy, so as to collect a body of statistics which will be sufficient and unimpeachable.

It is to be regretted that Prof. M'Intosh has rearranged the statistics according to his own ideas in a method which will probably not be accepted by the Board; and we fear that in some cases errors have crept in during copying and rearrangement which will make it necessary to have the figures carefully examined and verified before conclusions drawn from them can be trusted. We would ask Prof. M'Intosh whether the table on p. 196 does not contain an error of about 20,000 cwt. of cod in one year's work of the liners of the Moray Firth. We believe there are other errors in the same table. We have not had time to compare all the others with the original statistics, but it is evident that the tables must be received and used with caution.

The "Introduction" to the book gives a general statement of facts which, Prof. M'Intosh believes, "point to the conclusion that, with some exceptions, the fauna of the open sea, from its nature and environment, would appear to a large extent to be independent of man's influence." Well, that somewhat guarded statement may be difficult to contradict, and yet it gives an impression which we believe to be incorrect. So much depends upon what the "some exceptions" are, and upon what is meant by "open sea," "would appear," and "to a large extent." If the open sea means, for example, the North Atlantic, then we agree; but our fisheries are not there, and no one proposes to interfere with "Nature" in that wide expanse.

In the second chapter such important questions as the influence of trawlers and liners respectively upon the sea-bottom and the food supplies of fishes is discussed, and the results of the scientific work and the recommendations of the Royal Commission are fully considered. It is useful to have all these points collected and focussed by one who is so thoroughly familiar with them; but why stop short at the Commission of 1884, and not go on to consider the work of the more recent Select Committee in 1893? After all that we come to the statistics of the Fishery Board, arranged under the headings St. Andrews Bay, Firth of Forth, Moray Firth, and Firth of Clyde. Throughout the author seems attempting to demonstrate that the statements as to over-fishing have been exaggerated, that no denuding or exhaustion of the fishing grounds is taking place, or need be feared; and yet every here and there we meet with passages that suggest that he is himself only half convinced, or that he might possibly find arguments against his own contention. Surely the following are admissions of what he elsewhere denies:—"While, therefore, the present statistics show no serious diminution, it may be truly said that the total is kept up only by the supplies from Iceland, Faröe, and the Great Fisher Bank" (p. 100). "All that need be said at present is that, so far as can be ascertained, it would not appear that the closure of the inshore waters has made any marked increase in the fishes of the offshore waters. . . . No change, however, could be expected if the scarcity were due to general over-fishing" (p. 101). "And while the fishes may not, taking a broad survey of them, be very much reduced, and the totals in the market even increase, yet each ship and boat probably secures much less than in former times" (p. 102). "Instead, however, of placing the onus of the scarcity on any method of fishing, such as trawling, it would be more reasonable to lay the blame on general over-fishing" (p. 228). Even Dr. M'Intosh's phrases in regard to the prosperity of the fisheries, and their vast powers of recuperation, do not suggest too much confidence in the belief which he advances. He uses such terms as "no grounds for despairing," "condition is not unsatisfactory," "does not give rise to dissatisfaction," which somehow do not carry conviction.

This (the main) part of the book, giving the results of the closure experiments, conveys to the careful reader the impression that the author has turned the Fishery Board statistics inside out, gutted them, squeezed them and sucked them dry—if anything, even statistics, will stand such mixed treatment—with the result that he proves nothing except the insufficiency of the original material.

We must point out, however, that in his discussion and rearrangement of these statistics Prof. M'Intosh seems to have mixed up two very different things —the results from the closed areas and from the stations which were not closed. In this way he imports into the comparison of the conditions before and after closure a series of facts which have no right there. It is scarcely fair to draw conclusions as to the results of the closure from statistics of grounds some of which have never been closed.

In any remarks that are made as to the insufficiency of the *Garland's* statistics, it must be remembered that many valuable observations of other kinds have been made by the vessel and the scientific staff on board; and we should have liked to have seen in this book a little more acknowledgment of the important work done year after year both for the fisheries and for science by Dr. Fulton and by Mr. Thomas Scott.

The final chapter, "Summary and Conclusions," is the most interesting to read, and with much of it every biologist will agree. We have alluded to some of the chief points in the preceding paragraphs. Here and there we meet with severely sarcastic passages directed against the "practical" gentleman and the "fisherman's friend." Who are they, and what have they done? Is science not practical, and is Prof. M'Intosh himself not the most practical of all those who have had to do with the regulation of fisheries? And, again, who has such

claims to be regarded as the "friend" of the fisherman (*of to-day*) as he who has such trust in the "Resources of the Sea" that he would remove all restrictions upon fishing? Was it not Prof. M'Intosh who suggested a few years ago to the Select Committee that the North Sea should be divided into four portions, one of which should be closed each year against fishing? What is the Moray Firth compared with a fourth of the North Sea!

The book is admirably got up. The printing and the illustrations (views of St. Andrews and of fishing life) are excellent, and we have noticed wonderfully few misprints or mistakes:—"Machig" bay (p. 211) we take to be Mauchrie; and we believe Davaar to be the more correct spelling of the island off Campbeltown; "the late Mr. Spencer Walpole," referred to several times in these terms, is, we are glad to say, still with us as Sir Spencer Walpole, K.C.B.

Scottish zoologists have long regarded Professor M'Intosh as their leader, and as a pioneer in the application of science to the fisheries. This fresh work from his ever active hand and head will be received with gratitude and admiration even by those who cannot entirely agree with all its conclusions. L.

## ANOTHER BOOK ON BIRDS.

Birds: The Cambridge Natural History, vol. ix. By A. H. Evans, M.A., Clare College, Cambridge. 8vo, pp. xvi. + 635, with 144 figs. London: Macmillan and Company, 1899. Price 17s. net.

To write a book on birds is, one would imagine, the easiest of tasks, to judge at least from the steady and constant stream that pours forth year by year from the various publishing houses of this country. Nevertheless, though many are published but few are chosen. The latest claimant to our attention is one of the "Cambridge Natural History" series, and is, in some respects, a useful book. In the preface Mr. Evans tells us that "he has essayed the difficult and apparently unattempted task of including within some six hundred pages a short description of the majority of the forms in many of the Families, and of the most typical or important of the innumerable species included in the large Passerine Order." This is one of many statements to which we shall have to refer, as showing a lack of appreciation of the work of contemporary writers. In the "Royal Natural History," published some three or four years ago, there appeared a volume on birds in every way equal to the present work, and, in many respects, better.

The general plan of this work resembles that of Flower and Lydekker's "Mammals: Living and Extinct." It is divided into seven chapters, the first being introductory, and the rest devoted to the general descriptive matter. The frame-work upon which it is built is a good one, the classification adopted being that of Dr. Gadow. For the facts with which it is crammed the author is largely indebted to the "Dictionary of Birds," and Dr. Gadow's second volume on the Classification of Birds in Bronn's *Thierreich*.

In the general scheme of the systematic portion of the book it was apparently intended to prefix to each group a brief survey of the salient features of the external characters; those eventually selected are such as the number of toes, form of the beak, the presence or absence of an aftershaft, and so forth, added to which there frequently occurs the somewhat inconsequent statement that "the syrinx is tracheo-bronchial, and the furcula U-shaped," or otherwise, as the case may be, two isolated anatomical facts which seem singularly inappropriate and out of place here; moreover, they are often in addition, as statements of facts, incorrect.

In passing in review the account of the different groups, we notice many errors and many omissions. In the penguins, for instance, the tongue is said to be rudimentary (? vestigial). A moment's reference to the plates in Watson's magnificent monograph, published in the "Challenger" Reports, would have

shown this to be quite the reverse of true; but, strangely enough, this work is not referred to in this book. Although we are told that the syrinx is tracheo-bronchial, no mention is made of the median vertical septum running up the trachea in some penguins and in some petrels; moreover, there are *no* feathers in the penguin's wing which can be distinguished as primaries, there *are* apteria on the body, and the feathers are *not* scale-like. The number of times this last statement has to be contradicted is positively irritating.

No mention is made of the fact that in the grebes and some storks there are twelve primary remiges; on the contrary, the order Ciconia is definitely stated to have eleven. The adults and young of the order are said to "possess uniform down," whatever this may mean.

The nestlings of tinamous are birds "which run from the shell." The nestling-down is described as "simple, as in Ratite birds"; and this in spite of a paper in the "Ibis" for 1895, pp. 1-21, 507-9, on the pterylography of the tinamou, drawing attention to the fact, amongst other things, that the nestling-down of the tinamou was the most complex of all hitherto described. This, like that of the Anseres and Galli, as has been pointed out by the same writer, is further remarkable in having a large aftershaft rivalling that of the main shaft. The nestling-down of *Rhea* again has an aftershaft, a fact pointed out by Dr. Gadow in the "Dictionary of Birds," and which, strangely enough, Mr. Evans has overlooked. The curious succession of the remiges of the nestling Galli and of *Opisthocomus* find no mention in this book. But, enough, as to sins of omission.

"An introductory chapter," we are told in the preface, "has been written to meet the claims of the present day, on the external and to a limited extent on the internal structure of birds." We must refuse to believe that "the claims of the present day" will be satisfied with the meagre and often incorrect information to be gathered from the pages of this work, on the external and internal structure of birds.

In this introductory chapter, and throughout the book, the word "maxilla" is used for the whole upper jaw, an abuse of this word which is absolutely indefensible. The section devoted to feathers is very inadequate, even for this book. No mention is made of semiplumae, or filoplumae, or any distinction drawn between nestling-down and down feathers proper. The pterylosis of the wing is not touched upon, nor is any reference made to quinto- or aquinto-cubitalism, or the exceedingly interesting work on "Overlap" by Goodchild.

In describing the skeleton we look in vain for any reference to the forms of the palate, or of the nostrils (holo- and schizorhinism), or to the supra-orbital grooves so characteristic of groups such as the penguins, petrels, divers, gulls, and plovers. To describe the index digit of the penguin as fusing with the pollex is like talking of "the tail that wagged the dog!"

Two pages are devoted to terminology. The value of these is hard to see. Some words, such as allantois, amnion, procnemial (why not also ecto- and ento-cnemial) appear to occur nowhere else in the book. The definitions of amnion, broncho-tracheal and tracheo-bronchial syrinx, are all more or less unintelligible. Eleutherodactyl and syndactyl are not mentioned, though other forms of the avian foot are.

Although Mr. Evans has drawn so largely upon the "Dictionary of Birds" for the facts embodied in this compilation, he has caught nothing of its style. The crisp and elegant English of that book is here conspicuous by its absence. We cull the following from his description of the *Rhea*:—"The hens secured by each of the cocks lay together in a mere depression in the soil with very little, if any, lining; the eggs numbering from twenty to thirty . . ." And again, in describing the habits of the Norfolk plover, he writes:—"The mournful whistling cry . . . is chiefly heard at twilight, when the bird feeds upon worms, insects, molluscs, or even reptiles, frogs, and mice." And again, on the same page:—"The forms with almost uniform breasts, and a black patch or line over the eye." Surely we have a right to expect something better than this of a

book coming from one of our great universities. Nevertheless, on this volume Mr. Evans has undoubtedly expended much time and labour, and the information he must have gained during its compilation will stand him in good stead should a second edition ever be called for, in which case, if he will thoroughly revise the whole and extend the introductory part, he will succeed in making a really useful work.

The majority of the illustrations are by Mr. Lodge, and some of these are very good. W. P. PYCRAFT.

## GEOGRAPHICAL DISTRIBUTION OF PLANTS.

Pflanzen-Geographie auf physiologischer Grundlage. By Dr. A. F. W. SCHIMPER, Professor in Bonn University. 8vo, pp. xviii. + 876, with 502 plates and figures and four maps. Jena: Gustav Fischer, 1898. Price 27 Marks.

This charming volume will, we doubt not, prove one of the most useful numbers of the excellent botanical series which Messrs. Fischer have been issuing during the past few years. It is an able exposition of that phase of the science of plant-life, the study of which, till recently neglected, was brought to the front by the publication of Kerner von Marilaun's "Pflanzenleben." In brief, it is an account of the relation between plants and their environment and an explanation of plant geography in terms of these relations. It also affords a striking example of the value of good illustrations towards the elucidation of scientific facts. Such is the profusion of the plates, most of which are reproduced from photographs, and so excellent their character, that a student ignorant of German may gain a very fair idea of plant-life under varying conditions of climate or association by merely looking at the pictures. The labour involved in procuring so fine a series of photographs depicting vegetation "in the rough" in all parts of the world must indeed have been great; but Professor Schimper cannot but feel well satisfied with the result. Those who have seen the book will understand how impossible it is to select individual cases for honourable mention, and those who have not seen the book should embrace the first opportunity of becoming acquainted with its contents. There are rumours of an English translation. Generally speaking, we do not commend translations from the German for our students, but so exceptional a work may well supply an exception.

The subject-matter falls into three sections. Part I. enumerates the operating factors—Water, Temperature, Light, Air, Soil, and the Animal World. Each forms the title of a chapter in which is studied the response of plants in form and function to the condition in question. Part II. is called "Formations and Associations." Under the former title are discussed the types of vegetation brought about by climatic and soil conditions; under the latter the mutual relationships involved by a climbing, epiphytic, parasitic or saprophytic habit. Part III., occupying more than two-thirds of the whole, treats of Zones and Regions, and describes the plant life of the tropics, the temperate and arctic zones, and alpine and aquatic conditions.

At the end of each chapter is a list of books and papers dealing with the subject under treatment, and at the close of the volume are four excellent double-page maps illustrating the annual rainfall over the world and its seasonal distribution, and the distribution of the most important botanical formations in the world generally and North America in particular. A. B. R.

## EVOLUTION OF FRUITS.

Sketch of the Evolution of our Native Fruits. By L. H. Bailey. 8vo, pp. xvi. + 472, with 125 figures. New York: The Macmillan Co., 1898. Price 7s. 6d.

From Professor Bailey always something new! and moreover refreshing in its novelty, and put before us in so bright and interesting a manner that even the account of the origin of the numerous varieties and forms of cultivated fruits becomes a story fraught with interest and often with romance. With the history of the evolution of the fruits which are familiar to every citizen of the United States, the author has interwoven the life-story of the workers to whom is due the vastly improved condition of the fruit of the market as compared with its ancestor of the woods. And a great nation is much indebted to Professor Bailey for bringing into prominence, often at the expense of considerable work and trouble, the facts of the life of those who have rendered invaluable services in fields which are but little appreciated. His book is moreover a striking controversion of the argument that with so many good fruits and vegetables, the outcome of centuries of cultivation, it is waste of time for man to make new beginnings on the endemic products of a new continent. The story of the attempt to grow the Old World vine on New World soil is pathetic in its unbroken series of futile efforts, ruined prospects, and sometimes hearts broken by a succession of disappointments. But the story of the evolution of new kinds, in many ways superior to the European stocks, from the native American species, should be an inspiration to agriculturists working in a new country. While appealing more directly to our Transatlantic brothers, the book has a wide general interest, and in particular we would heartily recommend it to the botanist as a pleasant change from the more severe literature of his subject. A. B. R.

## ZOOLOGICAL RESULTS.

Zoological Results. Based on material from New Britain, New Guinea, Loyalty Islands, and elsewhere, collected during the Years 1895, 1896 and 1897, by Arthur Willey, D.Sc. Lond., Hon. M.A. Cantab. Part II. 4to, pp. 121-206, plates xii.-xxiii. Cambridge University Press, 1899. Price 12s. 6d.

The contributors to this part of Dr. Willey's "Results" are for the most part well-known authorities on the subjects on which they report. Of the eleven excellent plates illustrating the 74 pp. of text, we have besides lithographs, of which one is coloured, both photographs and photogravures, in each case probably as well as they could be done. This juxtaposition of the two last-named processes is of no small interest. The decision as to which method is preferable will of course depend upon the point of view, but the working zoologist can hardly fail to decide for the photographs, which will stand examination with a pocket-lens.

Confining ourselves to matters of general interest, we would call special attention to the first paper. It is by Prof. Hickson, and in it he shows us how to classify the Milleporae without the use of that indefinable word species. The various growth-forms are termed "facies." Instead of species "*complanata*" it is facies "*complanata*," and so on. This seems to assume some stability in the different forms, and that there are some laws of growth, both general and special, which when they can have free play result in definite recognisable shapes. If so, it is open to doubt whether the mere change of words is of sufficient importance to warrant its adoption. Apart from this criticism the paper itself suggests the question whether systematists generally might not

with advantage refuse to employ terms which they cannot clearly define. Although Prof. Hickson denies the existence of anything which corresponds to what he thinks to be the meaning of the word "species," only in *Millepora*, may we not go further and ask whether the haziness of the concept does not demand, in the interests of scientific thought, the general adoption of some more definable terminology?

As an illustration, on p. 175 Mr. Stanley Gardiner, who reports on the Stony Corals, is driven to express, and then immediately to suppress, a doubt as to whether *Cycloseris* and *Fungia* can be generically distinct. There is only one method of deciding such a point, viz. by extended comparisons; this will either reveal transition forms or else make it more and more improbable that any such exist. At present the systematic divisions into "genera" and "species" are, as the perplexed worker knows to his cost, too often little else than the result of guess-work. An unfortunate terminology stands in the way of precision. Having accepted the genealogical method in principle, is it not time that we ceased to limit it to the main stems and branches of the animal kingdom? Why not attempt to carry it to its logical conclusion and replace those nebular concepts "genera" and "species" by branching lines of differentiation? This, however complicated and difficult to work out, would, at least, admit of accurate treatment.

From this point of view it is gratifying to note that a new starfish is figured by Prof. Jeffrey Bell, and that he contents himself with calling attention to its leading features, and wisely refrains from endeavouring to give it a place in the system by guess-work. It must stand alone until fresh specimens help to link it on somewhere.

Mr. Beddard reports on the Earthworms and Mr. Shipley on the Sipunculids —and two new zoologists, Miss Isa Hiles and Mr. F. E. Bedford, appear on the field. The former describes the Gorgonacea and the latter the Holothurians. In welcoming them we cannot refrain from congratulating them on beginning their original work with systematics. The purely anatomical and morphological work of the laboratory course requires the starch taken out of it by actual experience of the extreme variability of organic life, and by learning that even the most stable of animal types is apparently quivering under the suppression, by the environment, of its power to change.

We congratulate Dr. Willey, and the band of able zoologists who are lending him their assistance, on the appearance of this solid instalment of the results of his travels. H. M. B.

## HISTORY OF BIOLOGY IN MINIATURE.

The Science of Life: An Outline of the History of Biology and its Recent Advances. By J. Arthur Thomson, M.A. 8vo, pp. x. + 246. (The Victorian Era Series.) London: Blackie and Son, Limited, 1899. Price 2s. 6d.

No more welcome book could be placed in the hands of the student of the history of science or of thought than this little volume on the "Science of Life." It is a review of all the scientific labours which centre in and bear upon the great phenomena and the problem of life: a history of biology in the widest sense. As such it fills a space which was empty before in the history of the sciences, though many suggestive essays or addresses by Huxley, Michael Foster, Burdon Sanderson in England, by du Bois Reymond, Claude Bernard and others abroad, have created a lively desire for a more comprehensive treatment of this fascinating subject. There are only two books with which we could compare the volume before us: the one is Sachs' "History of Botany," the other is Delage's great volume on the "Grands problèmes de la Biologie." Though we read the latter with interest and profit, we would recommend Mr. Thomson's small volume as a better guide through the tangled maze of biological reasoning.

In this country biology is a very modern science, and in popular opinion Darwinism or the doctrine of descent is the dominant conception which governs it. Mr. Thomson introduces us to the foreign and pre-Darwinian thinkers, Lamarck and Treviranus, who coined the term biology, and to those who formulated the idea of a comprehensive doctrine of life and of the economy in living nature: von Baer, Liebig and Boussingault, Johannes Müller, Claude Bernard, and many others. We are shown how the older natural history of Ray and Linnaeus became gradually unified and enlivened by the study of development, of the interdependence of natural beings, by the great generalisations of the cell theory, and finally by the theory of descent and evolution (Darwin and Spencer). We are shown the changes which the term "Vitalism" has undergone. It is not a paradox to say that, so long as the objects of nature were studied in their isolation, torn out of their natural environment and company, the study of living things was the most lifeless of all studies; it has only recently become a living study. How this has been brought about, the volume before us tells clearly, concisely, and attractively.

Special studies cease to be dry and mechanical when we are enabled to see the place and importance which they have in the great array of researches into the nature and significance of life. We heartily recommend the book alike to historians, philosophers, and biological specialists. British and foreign contributions are treated with equal impartiality; the orthography of foreign names—often faulty in similar books—seems very correct. We think we have noticed only one mistake, which, however, in the unfortunate absence of a complete index of names, we cannot at the moment refer to. J. Theo. Merz.

## SUCCESSFUL POPULARISATION.

Wonders of the Bird World. By R. Bowdler Sharpe, LL.D., F.L.S., &c. Assistant Keeper Sub-department Vertebrata, British Museum. With Illustrations by A. T. Elwes. 8vo, pp. xvi. + 397, 3 plates and 233 figs. London: Wells Gardner, Darton and Co., 1899. Price 6s. net.

Under this title Dr. Bowdler Sharpe gives us an acceptable and attractively written volume—an amplification of his popular lectures on the "Curiosities of Bird-life." A glance at the table of contents makes it manifest that among no class of animals do we find such multifarious "wonders" as among birds. Here we find chapters devoted to wonderful extinct and existing forms, to decoration, to playing grounds and gardens, to wonderful nests, to courtship and dancing, to mimicry and protective resemblance, to parasitic birds, to migration, and to geographical distribution.

Though the book is popular in its aims, yet it will prove useful to ornithologists as a work of reference, for Dr. Sharpe has gleaned his material from the vast and widely scattered fields of zoological and other literature.

In his treatment of the migration of birds, the author has scarcely done either himself or his subject justice. We are told, and it is quite true, that we have very much to learn concerning the wonders of migration. But it is equally true that considerable advance has been made in our knowledge during recent years, and that some highly important and interesting facts have been well established. To these no allusion is made. Something too might have been said regarding the marvellous journeys performed by migrants, almost equalling, as they do in some cases, an annual flight round the world. Other phases of this wonder of wonders of the bird-world might have been mentioned with advantage. This is, however, admittedly the imperfect chapter among many that are excellent. The book is nicely got up, well illustrated, and most reasonable in price. W. E. C.

## A FIRST INSTALMENT.

Applied Geology. By J. V. Elsden, B.Sc. Part I. 8vo, pp. 96, with 57 figs. London: "The Quarry" Publishing Co. Ltd., 1898. Price 5s.

For a very large proportion of the raw materials used in our industrial arts we are indebted, directly or indirectly, to mother earth; and hence a good work on practical geology, teaching us how to develop our mineral resources, ought to appeal to a wide circle. Mr. Elsden, in this rather slender volume, concerns himself with only a small corner of the subject; indeed, it may be doubted whether the issue of the work, in its present form, is not somewhat premature. The matter has already appeared serially in the columns of a very useful technical journal called *The Quarry*, where a continuation is now in course of publication, month after month. It would therefore, in our opinion, have been wiser to wait until the whole subject was completed, instead of reprinting the work in a fragmentary form.

So far, however, as the author goes in this first part, he may be followed by the student with much profit. A large proportion of the volume is devoted to instruction in geological surveying, and deals with problems connected with the dip, the strike, and the outcrop of strata. These subjects are necessarily of a mathematical character, but Mr. Elsden handles them in a simple and satisfactory fashion. The last chapter, forming 20 pages of the book, is descriptive of the principal stratified ore-deposits. Here the writer deals, first, with such detrital deposits as those containing gold, platinum, and tin-stone; then with ores precipitated from a state of chemical solution, such as bog iron-ore, clay ironstone, manganese-ores, and bauxite; and, finally, he describes those deposits in which the ores are disseminated through sedimentary rocks. It is in the last section that attention is directed to such important deposits as the copper-shales of the Permian beds, the metalliferous sandstones of the Trias, and above all else the famous "banket," or auriferous conglomerate of the Transvaal.

The work is illustrated by a number of figures which, though clear, are in many cases rather coarsely executed, and are needlessly large for so small a book.

## A FIFTEENTH EDITION.

Kirkes' Handbook of Physiology. By W. D. Halliburton, M.D. Fifteenth Edition. Pp. xxiv. + 872. London: John Murray, 1899. Price 14s.

When a class text-book has reached its fifteenth edition and is so largely used by the students of the present day as this compact volume is, then it must fulfil the requirements of its patrons. Probably the reason for this is that it takes up the whole subject of physiology, including also histology, so that a student obtains in a single volume practically all that he requires for examination purposes. The author has spared no pains in keeping it abreast of the most recent research in physiology. Every chapter shows not only signs of revision, but important matter has been added, such as Langley's recent work on the union of vagus and cervical sympathetic, Hammarsten's researches on blood coagulation, and those of Kossel on the constitution of the proteids. There are also numerous other additions, such as Sherrington's work on the reciprocal action of antagonistic muscles, and Haldane's researches on methaemoglobin. Here and there slight errors have escaped correction, as, for example, the definition of the posterior chamber of the eye given on page 738.

It is, however, a reliable general text-book. There can be no doubt that any student, who reads it carefully, will attain a good grasp of modern physiology. T. H. Milroy.

## AN AMERICAN INTRODUCTION TO AGRICULTURE.

The Principles of Agriculture. Edited by L. H. Bailey. 8vo, pp. xvi. + 300, with 92 figures. New York: The Macmillan Co. London: Macmillan and Co., 1898. Price 4s. 6d.

This is a book of unequal merit, better in the conception than the execution. Designed for Americans, it is written in the American language, and its "roiley streams" and "fitted fields" will hardly be recognised by English readers. Work done outside America is ignored, nor does any English book find a place in the bibliographical references. Opinions may differ as regards the wisdom of this, but there can be no difference of opinion in regard to matters of fact. There is manifestly a double error in the statement that haws are mixed with sand in autumn and sown in the following spring, the operation being known as "stratification." One wonders how the thousands of sheep annually fed on pasture or turnips, without access to water, manage to exist, if "no food contains so much water that it can be used by the animal to supply its needs both for water and solid matters"; and what have Scottish dairy farmers to say to the statement that "cattle should not be fed (*sic*) hay composed wholly of timothy and similar grasses"? The frequency of such errors makes the book unsuitable for students. W. S.

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The *American Geologist* for January 1899 is remarkable for its review of the life and work of Prof. E. D. Cope, written by Helen Dean King of Bryn Mawr, Pennsylvania. The bibliography, extending to 815 entries, will be of service to every naturalist. It is noteworthy that almost the whole of Cope's scientific contributions, from 1859 to his death in 1897, should have been devoted to the vertebrata. Few naturalists of his time, and probably none in the first half of this century, could show such a concentration of energy, combined with so large a literary output.

In the same number Mr. Manson publishes his "Laws of Climatic Evolution," in which he urges that an Ice Age occurs in the history of a cooling planet, at the period when the internal heat ceases to affect the surface-temperatures, but when the vapour from the warm oceans still shields the earth from solar heat. As this vapour is deposited in the form of snow upon the land-surfaces, which cool most rapidly, glacial conditions result, followed by a steady warming of the earth, as the atmosphere becomes free from clouds. In time, as the oceans become warmed by the sun, the new cloud-layers tend to check the general rise of temperature. The old arguments about the uniformity of tropical conditions in Mesozoic and Palaeozoic times are put forward as beyond dispute; and the numerous authorities quoted give the essay a more magisterial air than it might otherwise possess.

In the January number of the *Psychological Review* (vol. vi. No 1), besides an interesting address by Prof. Münsterberg on "Psychology and History," and an elaborate paper entitled "A study of the relations between certain organic processes and consciousness," dealing mainly with circulation and respiration, Mrs. Ladd Franklin contributes an admirable *résumé* of Prof. G. E. Müller's theory of the Light-sense. This theory has some genetic relationship to that of Hering, but is worked out on independent lines and with different postulates. For the antagonistic processes of assimilation and dissimilation Prof. Müller substitutes the conception of reversible chemical action. We recommend all those interested in fascinating and difficult problems of colour vision to peruse this summary of Prof. Müller's thesis.

The *American Journal of Psychology* for last October (vol. x. No 1) contains an elaborate and, it would seem, carefully conducted investigation on the applicability of Weber's Law to Smell (E. A. M'C. Gamble). The subject is too technical to render a brief description of methods and results possible. We must content ourselves, therefore, with directing the attention of those interested

in psycho-physical problems to Miss Gamble's paper, in which she claims to offer some evidence that Weber's law applies to smell and that the value of $\frac{\Delta r}{r}$ lies between $\frac{1}{4}$ and $\frac{1}{5}$.

In the April number of *The Naturalist* there is an interesting article by Dr. P. Q. Keegan on the physiological changes associated with the bursting of the buds in spring. The numerous buds which are distinguished by a pinky-red colour "contain apparently all along, and from the first moment of their existence, a certain quantity of tannic chromogen ready formed; all other leaflets contain merely the tannoid quercetin, or one of its allies, whose presence may possibly influence to some extent the tint of its infantile drapery." The deep brown of the opening leaves in the cherry, etc., is apparently due to tyrosine, a decomposition product of albumin.

In a paper on the making of the land in England, which appears in the *Journal of the Royal Agricultural Society*, Mr. Albert Pell returns to a subject which he has previously discussed. He describes the recovery of an area of 1350 acres of land and swamp which was in a state of nature as recently as 40 years ago, the enclosure award being dated December 19, 1861. The land was intercommonable of seven parishes, and was indicated on the Cambridgeshire map by the not very euphonious name of Grunty Fen. The whole story of the making of this land is told in a manner at once circumstantial and dramatic. This and other instances of a similar character which Mr. Pell has placed on record help to demonstrate the fact that the farm lands of England, before the cultivator or husbandman could turn a furrow or stock an acre, had first to undergo the process of manufacture at a large outlay of enterprise, money, and labour.

A useful publication, now in its fifth edition, is the *Naturalists' Directory*, published by Mr. L. Upcott Gill, and sold for one shilling. It contains the names and addresses of several thousand naturalists, and has been carefully edited. We notice, however, some strange omissions and wrong classifications, which suggest that it would be well for the publisher or editor to send proof copies for revision to experts in different departments, and to secretaries in important centres. Thus, under the heading *Edinburgh*, there is no mention of the Scottish Natural History Society, which is certainly one of the most vigorous of the many societies in that fair city.

The heading "Books of the Year" should either be made much better or dropped altogether. We do not see what 'Prantl and Vines,' for instance, has particularly to do with 1898, nor why sundry reprints from societies' proceedings should bulk so largely as they do. But additions and corrections for the next edition are cordially invited by the editor before November 25, 1899.

In the April number of *Knowledge*, Mr. Charles A. Mitchell, well known for his work on the evolution of bird-song, describes some instances of "love-gifts"; in other words, cases where a bird offers food to a courted companion or a brooding mate. Apart from fowl and pigeon, he describes the offering of love-gifts in the spotted flycatcher, the chaffinch, the robin, and the willow-wren. In the same number there are interesting articles by Prof. Arthur Thomson of Oxford on the anthropological data furnished by the hair, and by Prof. Grenville A. J. Cole on the fauna of the lowest Cambrian.

In the *Scientific American* of April 1, Prof. C. F. Holder has a short article on "Insect Migrations," in which, though dealing especially with locusts, he records a migration of yellow butterflies (*Colias*) that was remarkable for its duration and the vast numbers that formed the swarm. For three or four days Mr. Holder observed a continual stream of butterflies fluttering on, in the same direction to the north-east, and by personal observation he found that the flight of millions extended over 16 square miles, while reports from other centres made it certain that the area of migration was very much greater.

# OBITUARIES.

## OTHNIEL CHARLES MARSH.

Born October 29, 1831 ; Died March 18, 1899.

The death of Professor Marsh, of Yale, removes the last of the small band of pioneers in Vertebrate Palaeontology in North America. The Far West was becoming ready for scientific exploration at the time when he had completed an unusually extended college career, and his ample wealth enabled him to avail himself fully of his opportunities. He thus organised and led a series of expeditions which met with so much success in discovering the skeletons of extinct vertebrate animals, that the name of Marsh soon became a household word in connection with the recovery of wonderful lost creations. He died in the midst of work after only a week's illness, an attack of pneumonia proving fatal to his already enfeebled constitution.

Othniel Charles Marsh was born at Lockport, New York State, in 1831, and eventually entered Yale College, where he graduated in 1860. He remained for two more years engaged in geological and mineralogical studies, in the Sheffield Scientific School at Yale, and thence proceeded to Germany. He studied under Ehrenberg, Beyrich, Gegenbaur, Ferdinand Roemer, and others, in the universities of Berlin, Heidelberg, and Breslau. He returned to America in 1866, and became Professor of Palaeontology in Yale College, filling this honorary position, which was specially created for him, until the time of his lamented death.

Professor Marsh never lectured or gave systematic instruction in the college, but was able to devote the whole of his time, energy, and resources to the foundation of a Museum of Palaeontology and original researches connected therewith. He was a nephew of George Peabody, whom he induced to present to Yale a large sum of money for the foundation of the Peabody Museum of Natural History. One of the wings of this great institution was erected at once and became the scene of Professor Marsh's work ; the remainder has still to be built.

Until Marsh proceeded to the German universities, his chief interest centred in mineralogy, and his first paper, published in 1861, referred to the occurrence of gold in Nova Scotia. He wrote several other notes on minerals, and contributed to the fifth edition of Dana's "Mineralogy" in 1868. His studies in Germany, however, led him to direct special attention to palaeontology and comparative anatomy ; and he began the series of researches by which he is now best known. He first published a short description of two Labyrinthodont vertebrae from the Coal-Measures of Nova Scotia, assigning them to a new Ichthyosaurian Reptile (*Eosaurus acadianus*). He then contributed a series of notes, chiefly on invertebrate fossils, to the German Geological Society in 1864 and 1865. His first paper on fossil vertebrata from the West was a "Notice of a new and diminutive species of fossil Horse (*Equus parvulus*) from the Tertiary of Nebraska," published in the *American Journal of Science* in 1868.

After the latter date Professor Marsh himself began the remarkable series of explorations in the West to which we have referred; and henceforth he announced a constant succession of remarkable discoveries of extinct vertebrata, making known not merely new genera and families, but also several entirely new orders. He first discovered the Cretaceous toothed birds (Odontornithes), of which he published several preliminary notices and eventually (in 1880) a great monograph for the United States Geological Survey. Another series of preliminary notices of a race of early Tertiary mammals, of elephantine proportions, with paired horns on the head, culminated in a similar memoir on the Dinocerata in 1884. A constant succession of preliminary notices in the *American Journal of Science* relating to other remarkable extinct vertebrates were intended to be followed by further monographs published by the government, and a large number of plates were prepared for the purpose; but unfortunately circumstances prevented the completion of these works, and they must now be issued posthumously. Among other strange animals thus treated, may be mentioned the huge toothless flying reptiles (*Pteranodon*) of the Cretaceous period; the land reptiles (Dinosauria) of the Jurassic and Cretaceous periods, of which Professor Marsh discovered most of the known types and proposed the first scientific classification; the Cretaceous swimming reptiles, or Mosasauria; the Jurassic and Cretaceous mammals; the early ancestors of the horses; and several remarkable early groups of mammals, among which were the huge quadrupeds which he termed Brontotheria. To the last he displayed his usual activity, and his final paper on some Dinosaurian footprints appeared only in the number of the *American Journal of Science* current at the time of his death.

The main feature of Professor Marsh's work, judged from a scientific standpoint, consists in its concise mode of expression and profuse allowance of excellent illustration. Some of his critics, indeed, have said that the value of his publications depends more upon the artist than upon the Professor's pen. They contain very little generalisation and are never tainted by preconceived ideas such as sometimes distort more philosophical minds. They thus afford a mine of accurately-recorded facts which will ever be of permanent value, even when the present phase of biological thought and speculation has passed away.

Professor Marsh was well known in Europe as a frequent visitor, and had many interests in addition to those centred in palaeontology. He was an enthusiast in Japanese art, in many other kinds of art, in the growth of orchids, and in the care of his Alderney cows. His bachelor home in New Haven, now bequeathed to Yale, was replete with art treasures and books; and his garden extended over four ordinary "squares." He was a Foreign Member of the Geological Society of London, and received the first Bigsby Medal from that Society in 1877. In 1882 he was appointed Director of Palaeontology to the U.S. Geological Survey, and in 1883 he became Honorary Curator of Vertebrate Palaeontology in the National Museum, Washington. From 1883 to 1896 he was President of the National Academy. He was honorary Ph.D. of Heidelberg, and Correspondent of the Institute of France (Academy of Sciences), from which he received the Cuvier Prize two years ago. His great palaeontological collections were presented to Yale University by deed of gift on January 1, 1898. His will bequeaths the whole of his other property to the same university, of which he had been so long a conspicuous ornament.

## AXEL THEODOR VON GOËS.

Born July 3, 1835, Röks, Östergötland, Sweden;
Died August 1897, Stockholm.

Of so retiring a disposition was this excellent zoologist that even his death remained unnoticed for a year and a half by his colleagues in this country. It may not be too late for a few notes. The son of a nature-loving physician, Goës studied medicine at Upsala from 1854 to 1860, and in 1861 joined Torell's Spitzbergen expedition as naturalist and surgeon. On his return he joined Prof. Malmgren of Helsingfors in a research into the marine fauna around the Norwegian Finmark. In the summer of 1862, with a grant from the Swedish Academy of Sciences, he investigated the fauna off the west coast of Sweden. Having taken his licentiate examination at Upsala in 1864, he obtained the post of government and garrison physician to the former colony of Sweden on St. Barthelemy in the West Indies. Here he spent nearly five years, working on the fauna and flora of the Caribbean Sea, both from its greatest depths and from the shores of the island. His important collections are preserved in the State Natural History Museum at Stockholm. He returned to Sweden in 1870, bringing with him a wife, the daughter of the governor of the colony. Till 1895 he served as provincial physician in various districts, and then retired to pursue his natural history studies in Stockholm. As physician, Goës devoted much attention to the broader aspects of national hygiene, and was a strong advocate of the restriction of reproduction in those physically defective.

He was a devoted and able student of the Foraminifera, and his paper on this group from the Caribbean waters was a valuable contribution to the many attempts to bring into line the numerous variations to which authors had wrongly accorded specific rank.

Our information is mostly obtained from the Swedish medical magazine *Hälsovännen*, to which, as well as to *Eira*, *Land och Folk*, and *Ymer*, he contributed many articles of more general interest.

The deaths are also announced of Otto Böckeler, an investigator of the Cyperaceae, at Varel in Oldenburg, on March 5, in the 96th year of his age; at Rome, Count Abbé Francesco Castracane degli Antelminelli (1817-1899), one of the leading authorities on diatoms; Joseph J. Dowling, on February 2, at Foxrock, Dublin, a keen ornithologist and frequent contributor to the *Field*, and *Land and Water*, during the last twenty years; Sir Douglas Galton, K.C.B., F.R.S., D.C.L., LL.D. (1822-1899), general secretary of the British Association for 25 years, and president in 1895, a renowned authority and progressive worker in connection with sanitation; Otto Gelert, systematic botanist in Copenhagen, on March 20; Sylvanus Hanley, the well-known conchologist; John Lee, botanist, January 20, at the age of 49; Dr. Oliver Marcy, professor of natural history in North-Western University, Evanston, U.S.A.; Charles Naudin, member of the Paris Academy of Sciences, and believed by some to have anticipated Darwin as regards the idea of natural selection, at the age of 83; Dr. P. L. Rijke, of the University of Leyden, 86 years of age; on February 27, Dr. Gustav Schoch, docent in entomology in the Polytechnikum in Zürich; Joseph Stevens, for some years honorary curator of the Reading Museum, and a student of archaeology and geology, on April 7, at the age of 81; J. H. Wibbe, botanist, on January 7, at Schenectady, N.Y., at the age of 60; Franz Woenig, the botanist, at Leipzig; Surgeon-Major George Charles Wallich, a pioneer in deep-sea exploration, on March 31, in his 84th year.

# CORRESPONDENCE.

## PROFESSOR MORGAN ON MR. HERBERT SPENCER.

I HAVE been considering Professor C. Lloyd Morgan's critique (*Nat. Sci.* December 1898) of Mr. Herbert Spencer's *Biology*. Therein the dynamic element in life, by which is meant an underlying "principle of activity," seems to occupy a considerable amount of attention. Is this principle inherent in organic matter? Mr. Spencer thinks it is; but how are we to realise the fact? That is to say, how are we to account for the "origin" of what is known as vitalised matter? how can the chasm which separates living from dead matter be bridged? Mr. Spencer admits, on the one hand, that the processes which go on in living things are incomprehensible as results of any physical actions known to us; and, on the other hand, he must assume that living matter originated during a long stage of progressive cooling, in which occurred the formation of molecules more and more heterogeneous. Mr. Spencer here makes an assumption which is quite gratuitous, but it stands to his credit that he proceeds no further in the business. He practically admits that the heterogeneous molecules have got nothing to do with the definite combination of heterogeneous changes, etc., which enter into his conception of life. He owns that in this direction our explanations finally bring us face to face with the inexplicable, and there the matter rests. But Professor Morgan is not quite satisfied. With true journalistic instinct he fears that Mr. Spencer is false to the evolution which he has so eagerly and lavishly professed, and therefore a reference to the peculiarly Teutonic poetico-mystical distinction between the noumenon and the phenomenon is deemed advisable and unhesitatingly called into requisition. If we only could positively cognize "things in themselves," the Professor seems to imagine, then we should be able to fully and clearly understand how it has come to pass that the "more and more heterogeneous molecules" of dead matter suddenly jumped as it were into life, and we should be able to rigidly measure and estimate the resultants of the properties of the component earthy matter, etc. Evolution itself comes within the same sweep. "We formulate the laws of evolution in terms of antecedence and sequence; we also refer these laws to an underlying cause, the noumenal mode of action of which is inexplicable." That is to say, the cause of evolution as a process in nature is referred to the noumenon "the thing in itself," which is merely a matter of supposition, a something which we do not scientifically know, but can only suppose by dint of a high-strung imagination.

Who would now be rash in asserting that the erstwhile favourite pet fad of our most talented poets, literates, and journalists had not suffered degradation when it is compelled to fall back, so to speak, on the abstruse casuistry of German metaphysics? There is a certain mental condition, as Wordsworth well knew, which is barely able to separate and distinguish between the mind within and the matter without, which is not sure whether, for instance, a post met with on the roadside is inside one's head or outside of it. Now, this is just the sort

of poetical idealism which calls in the "noumenon" as a basis for what is apparently only "phenomenonly" seen. The true scientific metaphysics has no need of this poetic distinction. Matter is directly known in itself as possessed of qualities; there is nothing really behind it. The phenomena of the world, as reviewed by science, are not merely formulated in terms of antecedence and sequence; they are really considered as standing in the relation of cause and effect, which is an intuitive judgment underlying a portion of our empirical knowledge. There is no reference whatever to a supposititious underlying cause, nor do we know or understand anything whatsoever about the noumenal mode of action of any cause; all this is truly inexplicable. It is hoped, on behalf of the progress of science in this country, that Mr. Herbert Spencer's "inexplicable" may be interpreted in some such way as here suggested. All who recognise in the Darwinian labours an heroic but vain attempt to harmonise a rising and aggressive science with the best interests of poetry, literature, and journalism (all of which are excellent in their way), will no doubt enthusiastically join in this hope.

P. Q. KEEGAN.

PATTERDALE, WESTMORELAND.

## BEES AND FLOWERS.

Will Mr. Bulman forgive me for saying that his paper in your February issue, pp. 128-130, is spoiled by its vagueness and even inaccuracy in regard to facts? No discussion about bees in general, as if they were all of one kind, can be of much value in this connection; nor do observations on the honey bee prove anything about wild bees. I have now for several years been observing the habits of bees in New Mexico, and can affirm that there are numerous species which ordinarily visit only one kind of flower; notably nearly all those of the large genus *Perdita*. Others confine themselves to one family, as Cucurbitaceae or Compositae. Others, again, visit many flowers of very diverse families. The detailed facts have been published, so I will not occupy your space by repeating them; they will be found in *Proc. Acad. Nat. Sci. Philadelphia*, Jan. 1896, pp. 25-107; *Botanical Gazette*, Aug. 1897, pp. 104-107; *Zoologist*, Feb. 1898, pp. 78-81, July 1898, pp. 311-314; and *Bull. Sci. Lab. Denison Univ.*, xi. (1898), pp. 41-73. These references merely relate to my own observations, and are cited in defence of the statements above made; but of course there is abundant testimony from other sources.

T. D. A. COCKERELL.

NEW MEXICO COLLEGE OF AGRICULTURE AND MECHANIC ARTS AND AGRICULTURAL EXPERIMENT STATION,
MESILLA PARK, N.M., *March* 3, 1899.

## PAGINATION.

SIR—Referring to your kind notice of our latest Museum Handbook on page 252 of the March Number, you will, I hope, forgive my expressing surprise that a journal so scrupulous in matters of bibliography as *Natural Science* should suggest a double pagination in a reprint. Such a complication gives a needless uncertainty to references, and it is difficult to see what good is gained by it. If it can be shown that the numbers 1, 2, 3 present any advantage over 97, 98, 99, then I will recommend my Committee to use a double pagination, but not before. The question was carefully considered, and it was not the additional cost which determined the action of the Committee.

WM. E. HOYLE,
*Keeper of the Museum.*

OWENS COLLEGE, MANCHESTER,
*March* 17, 1899.

# NEWS.

The following appointments have recently been made:—J. H. Burkill, as assistant to the director of Kew; Prof. W. M. Davis, who holds the chair of physical geography in Harvard University, to be Sturgis Hooper professor of geology in the same university; J. H. Holland, as director of the botanical gardens in Calabar; Dr. Robert Muir, professor of pathology in University College, Dundee, to be professor of pathology in the University of Glasgow: C. C. H. Pearson, on the Kew staff, as assistant for India; A. J. Pieters, to succeed H. Hicks as first assistant botanist in the department of agriculture, Washington; Dr. Daniele Rosa, of the Zoological Museum in Turin, as professor extraordinarius of zoology in the University of Sassari; Mr. J. Arthur Thomson, M.A., lecturer on zoology in the School of Medicine, Edinburgh, to be regius professor of natural history in the University of Aberdeen, in succession to the late Prof. H. Alleyne Nicholson.

The Earl of Kimberley succeeds the late Lord Herschell as chancellor of the University of London.

The Smithsonian Institution have made the first award of the Hodgkins gold medal to Prof. James Dewar, F.R.S., in recognition of his researches on the liquefaction of air.

Prof. Milne Edwards, director of the Paris Natural History Museum, has received the Grand Cross of the Swedish Order of the Polar Star from King Oscar II.

The Paris Academy of Sciences has awarded half of the Lallemand prize to Mr. E. P. Allis for his memoir on the head of *Amia*, one of the Ganoid fishes.

A committee has been appointed for the purpose of presenting Dr. Richard Garnett, the late keeper of the printed books at the British Museum, with his portrait, as a mark of esteem and gratitude for his devotion to literature.

Dr. William Selby Church, senior physician to St. Bartholomew's Hospital, was elected president of the Royal College of Physicians of London, on March 27, in the room of Sir Samuel Wilks, Bart., retired. Dr. Church was Harveian orator in 1895.

The members of the Palaeontological Society have decided to express their appreciation of the services of their honorary secretary and editor, the Rev. Thomas Wiltshire, by presenting him with a testimonial. Subscriptions are not limited to members of the society, and will be received by the treasurer, Mr. R. Etheridge, F.R.S.

We learn from the *American Naturalist* that Prof. A. S. Packard, of Brown University, who has been wintering in the Mediterranean countries, is collecting materials for a life of Lamarck.

The *Times* of March 29 notes that another important advance has been made in the movement initiated by the Royal Geographical Society fifteen years ago, for the improvement of geographical education in this country, and the

recognition of the subject as a field of investigation of the first importance. One satisfactory result of this movement was the establishment, some twelve years ago, of Readerships in Geography at Oxford and Cambridge. These Readerships were substantially subsidised by the Royal Geographical Society. At the end of ten years the subject had obtained so firm a hold at Oxford through the activity of the Reader—Mr. H. J. Mackinder—that the University undertook to maintain the chair without further assistance from the society. Owing to certain local circumstances which it is unnecessary to specify, matters have not advanced quite so far at Cambridge, though the present Reader—Mr. Yule Oldham—is doing excellent work, which has been appreciated at the University.

At Oxford, as has been stated, a further step forward has been taken. Hitherto Mr. Mackinder has performed the ordinary functions of a Reader, which consist mainly in giving a certain number of lectures in each term; but it has been realised that the utility of the subject in various directions is so great that something more is now required. The result is, that there is little reason to doubt that before the end of the year a fully equipped School of Geography, or a Geographical Institute, will be established at Oxford, under the superintendence of the Reader, Mr. Mackinder. The Royal Geographical Society has offered £400 a year, for five years, towards the maintenance of this school, on condition that the University contribnte an equal sum. The delegates of the Common University Fund have agreed to contribute £300 towards the University's share, and early in the Easter term the Curators of the University chest will be asked to add another £100, and there is every reason to believe that Congregation will approve the decree. The scheme will be under the supervision of a committee of eight,—four, with the addition of the vice-chancellor *ex officio*, to represent the University, and three the Royal Geographical Society. The Reader will act as director of the school, and will have an assistant, besides two lecturers who will deal with special aspects of the subject.

The late Prof. O. C. Marsh, who died at Newhaven, on March 18, is said to have left his entire estate to Yale University. It is thought that it will amount to $150,000. He has left his house and grounds to form a botanical garden for the University. It will be remembered that in January 1898, Prof. Marsh presented his magnificent palaeontological, geological, and ethnological collections to the University of Yale.

An anonymous offer of £25,000 has been made to Birmingham University, on condition that a total amount of £225,000 is reached in a year.

The Chelsea Physic-garden has been saved from the builder. We learn from the *Times* that "The garden has been handed over to the trustees of the London Parochial Charities, who have agreed to dedicate a sum of £800 yearly to its maintenance. Under the new scheme the garden is to be administered exclusively for the promotion of the study of botany, with especial reference to the requirements of general education, scientific instruction, and research in botany, including vegetable physiology, and instruction in technical pharmacology, as far as the culture of medical plants is concerned. The practical management of the garden will be vested in a committee formed of representatives nominated by the trustees of the London Parochial Charities, the Treasury, the Lord President of the Council, the Technical Education Board, the Royal Society, the Royal College of Physicians, the Society of Apothecaries, the Pharmaceutical Society, the London County Council, and the Senate of the University of London. Earl Cadogan, and his successors, as representing Sir Hans Sloane, who conveyed the garden in 1722 to the Apothecaries' Company in trust for the encouragement of botany, is also a member of the committee. The garden, which comprises nearly four acres, facing the Embankment, was founded about 1673 by the Apothecaries' Company, and held by them on lease until 1722, when it was conveyed to them by Sir Hans Sloane on the trusts already

mentioned, with a proviso that, if the conditions were not fulfilled, the land should be held by his heirs in trust for the Royal Society or College of Physicians to carry on the garden for the same purpose. The Apothecaries' Company have recently found difficulty in keeping the garden in a proper state of efficiency, and neither the Royal Society nor the College of Physicians were prepared to undertake the trust on the condition of providing funds for its maintenance. The only course then appeared to be to sell the garden and apply the purchase money on a *cy-près* object, but, happily, the trustees of the London Parochial Charities have come to the rescue and saved the garden."

It has been decided to execute various special works at Kew Gardens in the course of the present year, including new filter bed, estimated to cost £2700; experimental well near the river, £200; new frame for Alpine plants, £120, etc. A sum of £200 will be set apart for purchases for the museum.

We have received a polyglot advertisement of the Ghizeh Zoological Gardens, near Cairo. The collection already includes a fair representation of mammals, birds, and reptiles, including "mongeese" (a plural which we have never had the courage to venture). Among other attractions are over 6 miles of paths (of which over 3½ miles are paved with coloured mosaic), the grottoes erected in the time of Ismail Pasha, and over twenty bridges.

In view of the visit of the British Association to Glasgow in 1901, an effort is being made to draw up complete lists of the Fauna, Flora, and Geological features of the Clyde District. A Natural History sub-committee has been formed and the zoological part of the work has been portioned out among many workers. The convener is Prof. John Young, M.D., the University, Glasgow; the vice-convener, Prof. Malcolm Laurie, D.Sc., St. Mungo's College, Glasgow; and the secretary, Rev. G. A. Frank Knight, M.A., Almanarre, Garelochhead. Information is specially desired as to (1) Distribution of Species; (2) Papers in Magazines, Journals, and Transactions of Societies, which might otherwise be overlooked; and (3) Names of workers in the different departments who might be willing to assist.

The area, as arranged by the Committee to be overtaken, is—"the natural drainage area of the Clyde, and of all the sea lochs which form extensions of its estuary." The northern limit, therefore, is the watershed beyond the head of Loch Fyne, and the southern boundary has been defined as a line drawn between the Mull of Cantire and the most southerly point of Ayrshire. It is a big undertaking, but the number of co-operating naturalists is already large, and we may confidently look for a valuable handbook in 1901.

An international veterinary congress will be held at Baden, August 9–14, 1899. The members of the congress will consist of delegates from foreign countries, as well as from the German empire. On the programme it is announced that an attempt will be made to prepare a uniform anatomical nomenclature, of which there is much need.

Prof. William Somerville, of the Durham College of Science, who is well known as a strenuous representative of the science of agriculture, has made numerous interesting agricultural experiments in the north of England, the results of which have been recently published in a County Council Report.

Prof. Cossar Ewart gave three lectures on zebras and zebra hybrids at the Royal Institution in April, and among others to follow we may note those on geology, by Prof. W. J. Sollas; on the atmosphere, by Prof. Dewar; on water weeds, by Prof. L. C. Miall; on the structure of the brain in relation to its functions, by Dr. Frederick Walker Mott; on pictures produced on photographic plates in the dark, by Dr. William James Russell; on climbs and explorations in the Andes, by Sir William Martin Conway; and on the discovery of the future, by Mr. H. G. Wells.

The ninth University Extension Meeting at Oxford will be held from July 29 to August 23, and it is said (we have received no formal programme) that some of the lectures have been arranged to illustrate the progress of science between 1837 and 1871. There will be lectures by Professors Gotch, Poulton, Sollas, Miers; and by Messrs. G. C. Bourne, A. W. Brown, Arthur Ransome, G. J. Burch, H. N. Dickson, and Reginald Farrar.

The Annual Report of the Zoological Society of London for 1898 shows receipts £29,208, an increase of £495 over that for 1897. This is largely made up by the subscriptions, as there is a falling off in "admissions" and other items. On the "payments" side there is an increase in salaries and pensions and in food for animals, and a new and gratifying item, "Grants in aid of science" £200. Sir William Flower still remains president, we are glad to see, and Dr. John Anderson, W. E. De Winton, Dr. Charles Gatty, Sir Hugh Low, and Dr. Henry Woodward are the new members of council. On the committee of publication F. E. Beddard, Sir George Hampson, Bt., and A. D. Michael replace Herbert Druce, E. T. Newton, and Oldfield Thomas, retired. The *Transactions*, vol. xiv. part 5, has been issued.

The prizes to be awarded in 1900 by the Belgian Royal Academy are announced. In natural science the subjects for essays are:—the geological formations at Comblain au Pont; the physical modifications produced in minerals by pressure; the structure and development of the Platoda; the presence of a nucleus in the Schizophytes; and the Devonian flora of Belgium. The essays may be written in French or Flemish, and must be sent to the secretary before August 1, 1900. The prizes are gold medals of 600 francs value.

The Hull Scientific and Field Naturalists' Club has issued an attractive programme of meetings and excursions for the summer session. Some of the excursions are in connection with the Yorkshire Naturalists' Union, to which this society is affiliated.

The thirtieth annual meeting of the Norfolk and Norwich Naturalists' Society was held at the Castle Museum on March 28, the president, Mr. J. H. Gurney, F.Z.S., in the chair. Mr. J. T. Hotblack was elected president for the coming session. The retiring president gave an address and read a paper on "The Bearded Titmouse and the Norfolk Broads." This is one of the twenty species for which the British list is indebted to the productive county of Norfolk. Here it was discovered by Sir Thos. Browne, who, through Ray, brought it to notice in 1674. Practically, the bearded titmouse is limited at the present day to the Norfolk Broads district, an area twenty-five miles by thirteen, of which part is marsh, where it still breeds annually.

At the meeting of the Geologists' Association, April 7, a paper on "The Geology of Brittany, with special reference to the Whitsuntide excursion," was communicated by Charles Barrois, D.Sc.

We have received the Report of the Committee of the Bristol Museum for the two years from October 1, 1896, to September 30, 1898. It is announced that the valuable geological collections of the late curator, Edward Wilson, have been presented to the Museum by his surviving brother and sister. Various structural improvements have been made. The top floor has been furnished with a plastered ceiling, thus rendering it useful for the storage of specimens. Two workrooms have been erected in the yard. The zoological specimens are being remounted and exhibited in smaller cases, just as is being done in so many other museums. The collection of British Lepidoptera has been re-arranged and re-labelled, with the help and generosity of G. C. Griffiths. The specimens of local rocks have also been re-arranged. Special temporary exhibits have been brought together in connection with public holidays: the first, a collection of books, manuscripts, and pictures, bearing upon the life of

Thomas Chatterton, the poet; the second, a number of drawings representing different features of bygone Bristol. Other special exhibits of antiquities and geological specimens were arranged for the meeting of the British Association. The Report ends, as such reports are wont, with a complaint of overcrowding.

The tenth annual meeting and conference of the Museums Association will be held in Brighton in the first week of July. The Mayor of Brighton, Mr. A. J. Hawkes, is the president-elect, and Mr. B. Lomax of the Brighton Museum is local secretary.

£50,000 has been left to the Trustees of the British Museum for the extension and improvement of the Library and Reading Room, by the late Vincent Stuckey Lean of Clevedon. Bristol City Libraries benefit to the same amount, under the same will.

The large *Ichthyosaurus platyodon* lately received at the British Museum (Nat. Hist.) from Warwick, is now in place on the wall of the Gallery for Fossil Reptiles. Although the proposed re-arrangement of the Ichthyosauria is not yet complete, the new specimen can be well seen. It is more perfect than the older specimen of the same species, that being in part plaster. A feature of the general re-arrangement will be the introduction of outline sketches of the various genera showing the bones in natural positions, which one has seen at Brussels, and which is of great convenience to the student.

We learn from *The Scientific American* that Brooklyn is to have a children's museum located in the Bedford Park Museum. Prof. W. H. Goodyear, from whom the suggestion emanates, has suggested that the initial purchase should be the technological series prepared in Paris under the title Musée Scolaire. To this will be added collections illustrating natural history, and the museum will also serve as a model for schoolroom decoration.

At the meeting of the Royal Geographical Society on March 26, the president, Sir Clements Markham, said:—"In my opening address this session, in referring to the urgency of obtaining funds for an Antarctic expedition, I expressed my conviction that the spirit which influenced the patriotic adventurers of the Elizabethan age was still alive among us. Many good men and true have answered to my appeal; but I now have the pleasure of announcing to you an act of unselfish liberality which does, indeed, remind us of the merchant princes of the days of old. Our associate, Mr. L. W. Longstaff, a Fellow of this society of many years' standing, has subscribed a sum which virtually puts an end to our chief difficulty. We shall be enabled at least to equip an efficient expedition consisting of one vessel, and to co-operate with the Germans in the scientific exploration of the Antarctic regions. You will like me to read to you the letter I have received from Mr. L. W. Longstaff, from which you will learn the noble motives that have actuated him, and the munificence of his contribution:—

"To the President of the Royal Geographical Society.

"Dear Sir Clements Markham—Being convinced of the imperative need of the immediate preparation of a British expedition, I have the pleasure to inform you that I have this day paid to the credit of the National Antarctic Expedition, with Messrs. Cocks, Biddulph, & Co., the sum of £25,000, which, I trust, will meet the exigency of the case.

"Though my attainments are but slight, I have all my life been much interested in scientific matters; and, as a Fellow of our society for nearly thirty years, it gives me peculiar pleasure to be able thus to contribute towards the advancement of our knowledge of the planet on which we live.

"I am, dear Sir Clements, yours faithfully,

"Llewellyn W. Longstaff.

"Ridgelands, Wimbledon, March 22, 1899."

I will now ask the meeting to pass a cordial vote of thanks to Mr. L. W. Longstaff, which I shall have great pleasure in conveying to him." The vote of thanks was seconded by Lord Lister, who said, *inter alia*:—"The British Antarctic Expedition is now assured, and the disgrace which would have been attached to this country for not rising to this great occasion is prevented. I need hardly say how deeply grateful we all feel to the donor of this great gift. It is a matter of peculiar satisfaction to myself that he is the son of a distinguished member of the medical profession. I do not know whether Lord Iveagh's magnificent gift to the General Institute for Preventive Medicine may have had anything to do with suggesting the idea to Mr. Longstaff. We can only hope that donations of this splendid kind will be plentiful in the future." Sir Clements Markham, interviewed by the *Central News*, said the fund for the Antarctic Expedition now amounted to £40,000. That would be sufficient to fit out one ship, if the strictest economy were exercised. It was under consideration, he said, to have the vessel built in Norway, as a wooden one could be got cheaper there than in England. It must be a wooden one, because the principal object of the expedition was to study magnetic influences in Antarctic regions, and an iron vessel would upset the calculations as to the magnetism. No one had as yet been selected to take command.

The first part of the Fauna Hawaiiensis, or the Zoology of the Sandwich Isles, being results of the explorations instituted by the joint committee appointed by the Royal Society and the British Association, and carried on with the assistance of those bodies and of the trustees of the Bernice Pauahi Bishop Museum at Honolulu, is about to be issued by C. J. Clay and Sons, Cambridge University Press Warehouse.

This work is published by the committee and edited by their secretary, David Sharp, M.A., F.R.S., Curator in Zoology of the Museum of the University of Cambridge; when complete it will probably consist of two volumes, as it is intended to give an account, more or less full, of the whole of the land-fauna of the islands. This fauna is of very great interest, not only from its geographical isolation, but also on account of its being rapidly extirpated by the progress of cultivation, and the introduction of foreign species which prey upon, or consume the food of, the original natives—many of the latter being already extinct.

The first part, containing 122 pages Royal 4to, two plates and a map, includes the Hymenoptera Aculeata (bees, wasps, and ants), of which about 200 species, mostly undescribed, have been obtained, and is by R. C. L. Perkins, B.A., who has carried out the explorations under the direction of the committee and Prof. Auguste Forel.

Prof. Boyd Dawkins announces that the Kent Coal Exploration Company have struck the coal-measures at Ropersole Farm, Barham, midway between Canterbury and Dover. This is about 9 miles from the shaft at Dover. The coal was met with at about the same depth, and the boring will be continued so as to allow of a section being made which will give a comparison of the thickness of the seams with those of Dover.

Mr. J. Stanley Gardiner, recently elected Balfour student of the University of Cambridge, left Marseilles on April 5 for the Island of Minikoi, situated about half-way between the Maldive and Laccadive Islands. He is accompanied by Mr. L. Borradaile, of Selwyn College, and Mr. C. F. Cooper, of Trinity College, will join the expedition during the summer.

After spending about three months at Minikoi, it is proposed to start for the Maldive Islands about September, and employ the next six or seven months in surveying these islands. The expedition will enter at Male, the Sultan's capital, and subsequently sail all round the group.

The main object of Mr. Gardiner's expedition is to study the formation of the coral reefs and everything pertaining to them. Especial attention will be directed to the following points:—(1) The depth at which reef-building

organisms can live in sufficient luxuriance to build up the reef; (2) the food of the coral polypes, both in reference to Mr. Gardiner's discovery that certain of these give off oxygen, and in reference to the minute surface fauna; (3) the importance of currents both in hollowing out and filling in atolls, and also as affecting the distribution of life in the lagoons and on the reefs. The study of the inter-relationship of all the numerous organisms on a reef one to another will receive attention, and careful investigations will be carried on, by means of sounding apparatus and valved leads, as to the character of the sea bottom. Temperature observations at different depths, and samples of sea water, will also be taken.

The *Belgica*, with the members of the Belgian Antarctic Expedition under command of Lieutenant de Gerlache, which left Ostend in August 1897, arrived at Punta Arenas, Patagonia, on March 28, 1899. The vessel was for some time fast in the ice, but the expedition is said to have accomplished something. It visited Hughes Bay and Palmer's Land, and prepared a map of these regions; it obtained various geological specimens, and landed twenty times. It afterwards proceeded towards Alexanderland, and penetrated the ice-bound region to the westward. The extreme latitude reached was 71·36; longitude, 92.

Two members of the expedition died, viz. Lieutenants Danko and Wincke; the others are all well.

Dr. L. Buscalioni has set out for the Amazons to collect for the Botanical Museum at Rome.

The greater part of the funds required for the scientific exploration of the great African lakes, has now been provided, and Mr. J. E. S. Moore, the leader of the expedition which has been organised for the purpose, will leave immediately for British Central Africa. It is expected that the work will occupy two years, of which nine months will be given to the biological and physical exploration of Lake Tanganyika. The other lakes to be visited are Kion, Albert Edward, Nyanza, and the Albert Nyanza. It is hoped that the results of this mission will add largely to our knowledge of the zoology of the lakes in the Rift Valleys, and help to solve many problems in regard to the geology and past physical characteristics of this region of Africa.

The *Kronstadtski Viestnik* states that a Russo-Swedish scientific expedition will start this month for the Spitzbergen Archipelago. The Russians will be represented by Staff Captain Sergiebsky, the zoologist Vinitsky, Dr. Bunge, and the geologist and mining engineer, Chernysheff. They will go in the Libau icebreaker No. 2, and the Bakan, and join the Swedish party at Stockholm. The expedition intends to winter in Spitzbergen, the Russians at Edge Island, and the Swedes at Parry Island. The names of the Swedes and of their ships are not given.

It is announced (*Scientific American*) that the attractions of the Paris Exposition will include an artificial volcano at Grenelle. It will be 328 feet in height and 485 in diameter, but "the trip to the top will be made very agreeable," for the paths will pass by beautiful bowers and arbors. Vegetation is to be so arranged that visitors will pass from the splendid flora of the Mediterranean to the stunted shrubs found on craters. There will be eruptions at fixed hours, an imitation lava flow, and a reproduction of Dante's "Inferno." Surely.

# Natural Science

A Monthly Review of Scientific Progress

June 1899

## NOTES AND COMMENTS.

### Ornithological Amenities.

Mr. Eagle Clarke, whose work on bird migration has won for him a high reputation even beyond the wide circle of ornithologists, relates an interesting though deplorable experience in a recent paper on "Ushant as an ornithological station" (*Ibis*, April 1899, pp. 246-270). Ushant appeared to him to lie not only at the diverging point where all those migrants which traverse the southern (French) shores of the Channel and the western coast of France change their course to proceed east or south, according to the season, but also to lie in the course of those birds of passage which, having skirted the western and southern coast-lines of Britain, take their departure from our south-western shores and wing their way northward to their winter-quarters, reversing this route in spring. In other words, Ushant is "an islet amid the two main streams of that vast tide of migrants which flows northward in the spring, and ebbs southward in the autumn, along the shores of western Europe." Nothing appeared to be known concerning the island ornithologically, and it was therefore natural that Mr. Eagle Clarke and his friend Mr. T. G. Laidlaw should have looked forward with expectation to their visit to the island.

As a matter of fact, a remarkable and unexpected experience awaited them, which compelled them to fall back on Alderney. We shall let Mr. Eagle Clarke tell his story.

"At Ushant we had hoped to remain for several weeks, but we had barely been six days on the island when an immigrant in blue and white arrived: to wit, a sergeant of gendarmes (there are no police on the island). He incessantly dogged our footsteps at close quarters during our rambles, while our place of abode was under his surveillance early and late; and although we were not disposed at first to take any notice of his presence, the espionage at length became so intolerable that we reported the matter to the British Consul at Brest, and requested him to protest to the French authorities against the vexatious treatment to which we were being subjected. This the Consul most obligingly did, but his efforts were unavailing, for the authorities

informed the Consul that the gendarme had been sent to watch us by instructions from Paris.

"By the advice of the Consul, in order to escape from more serious unpleasantness, we quitted the island on the 17th of September and returned to Brest. Thus was our Ushant expedition wrecked.

"It may be well to state here that our Foreign Office had informed the French Government about our intended visit to Ushant, and its object, long before our advent on the island. Thus the treatment meted out to us is inexplicable."

Some weird light on the misadventure (though there were some results after all) is given in a footnote, which contains an extract from *La Patrie* to the effect that "the English are in the habit of visiting Ushant with a view to secure pilots well acquainted with those dangerous seas, and to bribe the islanders with British gold. Only last year, under the pretence of rewarding the islanders for their conduct in connection with the wreck of the *Drummond Castle*, they scattered a perfect golden shower over the islands. In short, our neighbours, in the time of peace, pave the way for the purchase of traitors in the time of war!" If it were not sad it would be absurd; and one cannot help feeling that though there may be diseases more fatal, there is none which can expect less sympathy than that of red-tape-worm.

## Marine Zoology in Jamaica.

WE have just received the "Annual Report on the Institute of Jamaica for the year ended 31st March 1898." The museum appears to be making good progress under the curatorship of Mr. J. E. Duerden. The Caribbean Sea Fisheries Development Syndicate has investigated Jamaican waters with a steam trawler, with a view to establishing a fishing industry on a large scale. Unfortunately the scarcity of fish and the coral nature of the sea-floor militate against this plan. The operations of the trawler have been recorded by the curator; and a large series of local sponges has been contributed by the syndicate; one massive specimen is two feet across. Mr. Duerden himself has collected an almost complete series of the thirty-five known species of Jamaican Actiniaria, a group on which he has published several papers. Miss Rathbun of the United States National Museum has identified many additions to the collection of Crustacea. It may be useful to note that a 3 to 5 per cent solution of formalin has now been in use for over two years as a preservative fluid for fishes, coelenterates, and holothurians, and has preserved the natural form and colours of the animals better than alcohol. Mr. Duerden has also been investigating the relics of the aboriginal Indian inhabitants of Jamaica, and the collection of these relics has been much increased. The Report indicates a gratifying growth of interest in the museum.

## An old Link between North America and Arctic Europe.

*Tetradium* is the name given by Dana to a Palaeozoic fossil of somewhat uncertain affinities, but apparently a coral. The presence of four septa, forming an aperture shaped like the diaper of Gothic architecture, and of grooves or rugae outside the tubes and exactly opposite the septa, are features reminiscent of the Rugosa. A certain connection of the calicular tubes through intervening lamellae induces a superficial resemblance to the chain-coral—*Halysites*. Some of the species grow in compact masses, the tubes in close contact; in others, such as *T. apertum*, the tubes are further apart and united laterally only by horizontal tubules such as occur in *Syringopora*, a genus from which it differs in all other respects.

*Tetradium* has hitherto been known only from the Ordovician of North America, being confined to the Trenton and Hudson River groups of Tennessee, Kentucky, Ohio, New York, and Canada. Attempts have, it is true, been made to record it from elsewhere. Nicholson and Etheridge described a *Tetradium peachi* from the Scottish Silurian, but it does not present the characters of the genus. Schlueter has shown that Frech's *Tetradium eifeliense* is only a *Calamopora*. Last year, however, Professor A. G. Nathorst brought back from Beeren Eiland, near Spitzbergen, fragments of limestone in which Professor Gustaf Lindström has detected a *Tetradium* indistinguishable from *T. apertum*, Safford. He describes and figures it in *Öfversigt af k. Vetenskaps-Akademiens Förhandlingar*, 1899, No. 2. This both extends the geographical range of the genus and proves the existence of Ordovician limestone on Beeren Eiland.

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## The Artillery of the Bombardier Beetles.

The ejection of an acid fluid, which explodes and vaporises in contact with the air, from the tail-end of carabid beetles of the genus *Brachinus* is well known to entomologists. It seems to serve as a valuable protection against the pursuit of larger members of the family. Dr. L. Bordas has recently (*Zool. Anzeiger*, xxii. pp. 73-76) studied the apparatus to which this secretion is due. A pair of glands, consisting of bundles of lobules, are situated on either side of the intestine beneath the dorsal body-wall; from these lead long, coiled efferent ducts, which are remarkable for the narrowness of their internal diameter and the great thickness of their circular muscle-layer which is arranged in a series of annular discs. Each duct opens into a large reservoir, whence an ejaculatory duct arises to open

a little above and behind the anus. Both these ducts and the reservoirs are lined with chitin and evidently formed by invagination of the ectoderm. The likeness of the discharge of these beetles to that of artillery is increased by Dr. Bordas' observation that after dark the emitted vapours show a phosphorescent light.

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## Economic Entomology.

WE have received Miss Ormerod's twenty-second report on Injurious Insects, and notice with satisfaction that a general index to her whole valuable series is in course of preparation by Mr. R. Newstead of Chester, who has undertaken to collaborate with her in the preparation of a second series of reports. In these a special feature is to be the utilisation of short notes on means of prevention and remedies, which may be furnished to Miss Ormerod by her correspondents, less space being devoted to descriptions and figures of insects already well known. Among less familiar pests mentioned in the present Report are the gall-midge, *Diplosis pyrivora*, Riley, whose maggot feeds within pears, causing a stunted and aborted growth; and the "rustic" moth *Hydroccia micacea*, Esp., whose caterpillar bores into potato stems.

From the U.S. Department of Agriculture, we have received Part 6 of the Bibliography of American Economic Entomology, comprising titles of publications from June 1888 to December 1896. This work will tell the student of insects of many papers which do not gain notice in the ordinary zoological bibliographies.

The latest bulletin (No. 19) of this Department is by Mr. F. H. Chittenden and deals with some insects injurious to garden and orchard crops. Of considerable biological interest is the ladybird beetle, *Epilachna borealis*, Fb. Unlike most of its family this insect is a vegetarian, and feeds largely, both in the larval and perfect stages, on the leaves of "squash" and pumpkin. It has the curious habit of marking out a circular area on the leaf within which it eats. A hemipterous insect (*Anasa tristis*, D. G.) which feeds on the same plants, and another (*A. armigera*, Say) which devours cucumber leaves, are carefully traced from the egg through five nymphal stages.

Mr. M. V. Slingerland of the Cornell University, Ithaca, N.Y., contributes to the *Photogram* (vol. vi. pp. 141-145) an instructive article on the application of photography to economic entomology. A want of definition in detail is often to be noticed in photographs of insects, but Mr. Slingerland's methods seem to give remarkably clear pictures. "Long exposures with all the light it is possible to get" and "a slow-developer" are recommended for the best results, and in many cases the use of a yellow ray filter greatly increases the effect.

# The Royal Society's Conversazione.

The first of the two annual conversaziones of the Royal Society was held on May 3. Among the exhibits of special interest to our readers the following may be recorded:—

The tsetze fly committee of the Royal Society exhibited enlarged photographs, taken by Surgeon-Major Bruce, illustrating districts in South Africa affected by the tsetze fly disease.

Prof. J. Norman Collie showed some fine photographs taken during a journey of exploration to the sources of the Athabasca and Saskatchewan rivers. The chief result of the expedition was the discovery of the largest snow-fields and highest peaks at present known to exist in the Canadian Rocky Mountains.

Mr. Saville-Kent showed some lantern slides of various zoological and botanical subjects reproduced in their natural colours by photography, prepared from negatives taken through screens corresponding in tint with the three primary colours of the spectrum—red, green, and blue-violet—as first suggested by the late Prof. Clerk Maxwell, and since adopted in the colour-photography researches and inventions with which the names of Ives, Joly, Lippmann, Lumière, and others are associated. The positives from the foregoing negatives are printed as three-film or glass transparencies, each of which is tinted with the colour complementary to that of its respective negative; they are then superimposed in correct register.

Messrs. J. E. Barnard and T. A. B. Carver demonstrated an elaborate photomicrographic apparatus for high powers, designed for the Jenner Institute of Preventive Medicine. To secure rigidity among the several components the apparatus is upon a stiff girder; which, in extreme cases, may be suspended by springs to isolate it from a source of vibration. The apparatus is arranged for use with transmitted light, but the illumination of opaque objects is provided for by a prism carried upon a jointed support above the stage, the arc lamp and condenser having sufficient range to allow the beam to be thrown in a line parallel to and above the line of collimation of the microscope. A useful feature is the graduation of the slides upon which the components move; thus the conditions under which any photograph is obtained may be recorded and reproduced. The length and rigidity of the apparatus permit of very great magnification without blurring. Valuable results have already been obtained with this, especially when aided by the differentiating action of monochromatic illumination.

At an adjoining table were to be seen numerous microscopic preparations of mosquitoes, exhibited by Dr. P. Manson and Surgeon-Major Ross, to show the development of the malarial parasites, *Filaria nocturna* and *Proteosoma grassii*.

Dr. G. H. Fowler exhibited various floating organisms obtained by him on board H.M.S. "Research" from the Faeroe Channel. The deep-water forms were taken by a net made to open and close when desired; they came from various depths down to 500 fathoms, the temperature being in some cases as low as 30° Fahr. Of chief interest were some beautiful specimens of Phacodarian Radiolaria, notably *Coeloplegma murrayanum;* these confirmed the wonderful drawings of Haeckel. It was curious to see among the salt-water captures an example of that parasite of fresh-water fish—*Argulus foliaceus.* Another noteworthy occurrence was that of some shallow-water Arctic forms found in the cold deep waters of the Faeroe Channel.

The Marine Biological Association attempted to illustrate the methods of feeding of marine animals by a series of selected animals from the neighbourhood of Plymouth. One or other of the following means is usually adopted: (*a*) the production of currents by either ciliary or muscular action; (*b*) the prehension of moving prey either by special organs provided with pincers, suckers, or nematocysts, or by teeth and jaws; (*c*) browsing upon or sucking in nutritive material, either living or dead. The Association also showed charts illustrating the distribution of the fauna and bottom-deposits near the 30 fathom line from the Eddystone Grounds to Start Point.

The collections made by Mr. C. W. Andrews in Christmas Island, and those more recently brought by Dr. H. O. Forbes and Mr. W. R. Ogilvie Grant from Socotra, attracted much attention; but to these we have previously alluded.

The skin of *Neomylodon* was exhibited by Dr. F. P. Moreno, who also showed a plaster reproduction of the skeleton of *Toxodon platensis* (see *Natural Science*, vol. xiii. p. 290), as well as many skulls of other extinct mammalia from Patagonia and Argentina.

Dr. H. Sorby advocates the use of strong glycerine for preserving the more translucent marine animals in museums. It makes the colours of the surface bright and renders much of the internal structure distinctly visible. Menthol is used by him to kill sea-anemones and similar marine animals, since the addition of a little to the sea-water in which the animals are living causes them to expand fully, and in many cases to die so displayed. They may then be transferred to 4 per cent formalin, and kept thus distended or mounted in balsam as lantern slides. Some of the latter were exhibited and commented on by Dr. Sorby.

Another short lecture was that by Mr. R. Kearton, who with Mr. C. Kearton has taken such admirable photographs of our native birds in their natural haunts.

Professor Arthur Thomson of Oxford considers that three factors are concerned in the production of curly hair: (1) the hair shaft, (2) the hair muscle, and (3) the sebaceous gland. Straight hair is always

circular in section, and is usually thicker than curly hair, which is ribbon-like and fine. In order that the muscle may act as an erector of the hair it is requisite that the shaft of the hair embedded in the skin should be sufficiently strong to resist any tendency to bend; unless this be so the lever-like action necessary to produce its erection is destroyed. When the hair is fine and ribbon-like, the shaft is not sufficiently stout to resist the strain of the muscle and naturally assumes a curve, the degree of curvature depending on the development of the muscle, the resistance of the hair, and the size of the sebaceous gland. The curve thus produced becomes permanent and affects the follicle in which the hair is developed; the softer cells at the root of the hair accommodate themselves to this curve, and, becoming more horny as they advance towards the surface, retain the form of the follicle; the cells on the concave side of the hair are more compressed than those on the convex side. In this way the hair retains the form of the follicle after it has escaped from it. Professor Thomson illustrated his views by a working model, but did not imagine that his ingenious mechanism could be applied to the human scalp.

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## Essex and East Anglia.

We have received Volume X. of the *Essex Naturalist*, and find that in it the praiseworthy traditions of the Essex Field Club are continued. The volume contains the contributions for the years 1897 and 1898. Since we cannot allude to all of them, we may select as among the more important the various papers on recent and Post-pliocene mollusca by W. M. Webb, A. S. Kennard, and B. B. Woodward, and "The Entomostraca of Epping Forest," by D. J. Scourfield. As these and other papers witness, this club wisely restricts its energy to its own field, the natural history of the county of Essex, thus eliciting from its members work of real value and cultivating that healthy local spirit which is the best preparation for national and international co-operation.

Such co-operation is already proposed. At a largely attended meeting at Witham, on July 23, 1898, the question of federation of the East Anglian Natural History Societies, previously ventilated by Sir Francis Boileau, Mr. G. S. Boulger, and others, was definitely put forward by Mr. W. Cole, the secretary of the Essex Field Club. It was unanimously resolved that there should be an annual conference of the East Anglian Societies, on the lines of that of the South Eastern Union to which we have so often referred. The first conference is to be held this year. The opinion was strongly expressed that one of the most valuable results to be looked for from such co-operation would be the establishment of a single natural history journal for the whole of

East Anglia. From the faunistic and geological points of view the counties of Norfolk, Suffolk, and Essex have much in common, and the concentration of all records in one periodical publication would be a gain to local field-workers, as well as to the weary student in the library. Such a body as the East Anglian federation would arouse more interest in natural science, would attract more members, would obtain more money, and would be listened to with greater respect than any local society however energetic. Mr. Cole, in his paper, alluded to several lines of activity in which such a federation could do good work. One such is the establishment of a biological station, and this suggestion has been welcomed warmly. Now there are already a fair number of marine biological stations around our coasts; but where this country falls behind so many others is in the absence of a fresh-water station. The broads of Norfolk offer an admirable field for the systematic study of fresh-water life, and we earnestly hope that the East Anglian naturalists may see their way to found such a station as we have long advocated.

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## The Essex Museum of Natural History.

THIS is another matter in which the Essex Field Club is doing good service. This museum must not be confused with the charming little Epping Forest Museum of which we have previously given some account. It is a far bigger affair and covers a wider field. The Club has long had a museum of county natural history at Chelmsford, but it was found impossible to carry this on in an adequate manner without financial aid. This was sought at Chelmsford from individuals, from a local society, and from the Corporation, but in vain. In June 1897 the Corporation of West Ham was approached, and in July 1898 an agreement was entered into between that body and the Essex Field Club. Towards the £6000 needed for building and site, Mr. J. Passmore Edwards contributed £2500, thus removing the only serious difficulty, and on October 6, 1898, he laid the foundation stone of what is to be called the Essex Local and Educational Museum of Natural History. The museum, designed by Messrs. Gibson and Russell, will ultimately be a very handsome structure, adjoining, but distinct from the Technical Institute of the West Ham Corporation in the (Main) Romford Road, Stratford. The balance of the cost, and the upkeep of the building will be defrayed by the Corporation of West Ham.

The promoters of this museum have availed themselves to the full of the experiences of others, and their scheme for the maintenance of the institution may serve as a model. This is shown by the following extracts from the statement and appeal now being circulated.

"The Corporation agree to dedicate the main portion of the building to the purposes of a Museum of Local (Essex) Natural History, Pre-Historic Archaeology and Anthropology, and of Educational series relating to the same; to warm, light, and provide for the caretaking of the building; that the Club shall have the sole scientific control of the collections and the appointment of the curator, and be allowed to keep its library in the building; the Corporation also agreeing to make a grant of not less than £100 per annum towards the curatorial expenses. The Club agree to place their county collections, cases, and cabinets in the Museum (excepting the Epping Forest collections, which are to be retained in the Forest Museum at Chingford); to do their best to increase and improve the same; to undertake the selection and scientific control of the collections; to raise a certain capital sum for the further equipment of the museum; to appoint a curator, and to devote a sum of £50 per annum towards the curatorial expenses." Thus there is assured an annual income of £150 for that most important part of a museum—the curator. "The museum will be a Local (Essex) one, supplemented by short series having an educational value, and designed to show the place of the local forms in the general scheme of classification of animal and vegetable organisms. The promoters aim at its eventually fulfilling three main purposes: (1) The instructive recreation of the ordinary visitor by means of carefully arranged sets of the chief forms of life inhabiting the district, and examples showing the nature and meaning of fossils and geological formations; (2) assisting students and field-naturalists in identifying and studying the groups in which they are interested; (3) collecting and preserving authentic series of all forms of life, recent and extinct, occurring in Essex, as well as geological and anthropological specimens. This is a matter of really great scientific importance in view of the changes in our fauna and flora now so rapidly being brought about by the increase of population and the consequent effacement of natural conditions in many parts of the country." This scheme will be adhered to rigidly. Incongruous miscellanea and donations saddled with conditions will be sternly rejected. On the other hand, there will be systematic and organised collecting. "In addition to the sets now in the possession of the Club, valuable collections have already been promised for the new museum, including a Herbarium of Essex plants, a large collection of fossils and rocks for the educational series, and a collection of the Crag fossils. Also a collection of the Pleistocene Mammalian fossils and sets of the Lepidoptera and Coleoptera of the county, etc. The main difficulty lies in the provision of suitable cases and cabinet accommodation for the collections, and the classification and arrangement of the same, which will entail a very considerable expenditure of *money* and *time*." This latter is the part of the Essex Field Club, which desires to raise for the purpose £1000, and has therefore started a "Museum Purchase

Fund" to which donations are solicited from all interested in the spread of natural science and of popular education. Donations may be paid to Lloyd's Bank (Essex Field Club Account—Town Side), 72 Lombard Street, E.C.; or to the Treasurer, W. C. Waller, Esq., Loughton, Essex. A point to which *Natural Science* has often drawn attention should be noted: "Under the agreement, should the Club at any future time be unable or unwilling to continue to control the museum, it will revert to the Corporation, to be by them maintained and managed precisely on the lines laid down in the agreement. This stipulation gives a warrant of permanence to the museum—a matter of the utmost importance."

It is hoped that the museum may be opened to the public in the spring of 1900.

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## Nature-Study.

WE are pleased to observe the regulation in the code of the Scotch Education Department for 1899 recommending the teaching of Nature Knowledge, and this on the basis of regional survey. This change has come as a surprise to teachers, but opportunities to prepare for it are already beginning to be offered them by educational authorities. In the past the only regular means available have been the museums and informal general excursions of the various local natural history societies, or the occasional school expeditions conducted by every effective science teacher. Among more organised efforts may be mentioned those of the School Board of Dunfermline with the help of Mr. Henry Beveridge of Pitreavie, who have for several years past arranged regular series of school excursions throughout the main centres of natural, historical, or social interest. These were at first conducted by Dr. Herbertson and Mr. T. R. Marr, and since by Mr. Robert Smith, B.Sc. During the past few years also the County Council of Perthshire has been specially active in organising central classes for the training of teachers in Physical and Natural Science, and the movement seems in a fair way to obtain fuller scope. One example of the quickened interest is to be seen in the trebling of the number of teachers attending the evening Botany Class at University College, Dundee.

In the same connection we note that Mr. Henry Coates of Perth has recently granted prizes to the best essays written by Perthshire school children on a visit to the admirable museum of the Perthshire Natural History Society. The essays were corrected by Mr. W. J. Calman, B.Sc., whose report, though at the same time encouraging, shows the very urgent need of a systematic extension of this mode of utilising local museums in school education; and of the necessity of teaching both child and teacher alike what and how to observe.

# Fishing at the Cape and elsewhere.

THE fishing industry at the Cape of Good Hope is passing through its life-history more rapidly than at home. Here we are well on in the stages of metamorphosis, and at the Cape the larval stage is just coming to an end. To be plain, the method of catching fish by a trawl has caused a rude awakening amongst the seafaring folk who, with the primitive methods of open boat and hand-lines, have been wont to supply the market to their own advantage. As in 1883 in Scotland, so in 1898 at the Cape, a Select Committee on the fishing industry sits to hear the same old "chestnuts" of destruction by the trawl of the fish-spawn, of fish-food, and of nearly everything else in the sea, but fortunately in the former case scientific knowledge was forthcoming, whereas in the latter the scientific voice seems to lack decisiveness.

Dr. Gilchrist, the Government Marine Biologist, states in evidence that "we know absolutely nothing about the spawn of the fish." This statement seems to require some explanation, considering that the author of it has been over three years in Cape waters, and that an experimental trawler at an initial outlay of £7000 and an annual expenditure of "between £3000 and £4000" has been placed at his disposal. From a distance it seems as if it should have been possible by this time to make sure of the leading facts relating to the spawning of nearly all the commonest food-fishes of the Cape.

Turning our attention to New South Wales we find the fisheries at an equally early stage of metamorphosis. Trawling experiments, as recently in Jamaica, are being conducted with a view to the introduction of this method of fishing into the colony. The experiments, unlike those in the West Indies, were distinctly successful. Mr. Frank Famell is to be congratulated upon an enterprising attempt to show the possibilities in this direction. His report is accompanied by a valuable and concise report upon the fishes which were obtained, drawn up by Mr. E. R. Waite. He describes a new species of Chimaera (*C. ogilvyi*) and of boar-fish (*Histioptuus famelli*), besides being enabled to definitely locate such types as the ordinary flying-fish. The six weeks' trawling cruise of H.M.C.S. "Thetis" opens up great possibilities for the Australian fisheries, but if there is a strong contingent of line fishermen on the sea-border of New South Wales Mr. Famell must be prepared for the inevitable consequence!

A short report upon the fish-fauna of Long Island has an interesting series of observations upon the attraction of fishes by lantern light. Three hundred and forty-eight species of fishes are now known from the New York district, of which one hundred and sixteen are from the fresh waters, and thirteen are anadromous in habit.

## Development of Lepidosiren.

A VALUABLE contribution to our knowledge of the Dipnoi will be found in Mr. Graham Kerr's paper on the external features in the development of *Lepidosiren paradoxa*, Fitz., communicated to the Royal Society, May 4. The abstract with which we have been favoured summarises the more important points as follows :—

The egg is very large, 6·5 to 7 mm. in diameter. It is surrounded by a special capsule, at first thick and almost jelly-like in appearance, later on (after fertilisation) thin and horny. Outside this was found in rare cases a thick jelly resembling that of the frog's egg. The egg is without a trace of dark pigment. Segmentation is complete, resembling most nearly that of the egg of *Amia*, and leads to a condition with an upper hemisphere of small cells with large segmentation cavity, and a lower of large yolk cells. Gastrulation begins with the appearance of a row of depressions, or a continuous groove along about one-third of the whole extent of the margin between small and large cells. During its progress the small-celled portion spreads over the lower yolk cells by the addition to its margin of small cells split off from the yolk cells. As the groove referred to deepens into a slit to form the archenteron, it becomes gradually shorter, and the eventually complete blastopore is a crescentic slit only about a quarter of the length of the original groove. The medullary folds soon appear running forwards from the blastopore. There is no trace externally of a blastoporic or protostomal seam running along the back between the medullary folds. The folds are low and inconspicuous, and they are continued into one another behind the blastopore, which becomes the anus. There are only slight traces of overarching of the medullary folds to enclose a neural canal. During the later stages of intraoval development, the posterior end of the body becomes much more conspicuously folded off the yolk than the head end. The *Lepidosiren* hatches out as a tadpole-shaped larva, still completely devoid of dark pigment. Just about the time of hatching the cloacal opening closes temporarily. As the larva develops it becomes extraordinarily amphibian-like. It possesses large pinnate external or somatic gills, four on each side, corresponding to branchial arches I., II., III., and IV. A large cement organ is also present, which during its early stages is of the characteristic crescent shape so usual in the embryos of Anura. Pigment begins to appear about ten days after hatching—first in the retina, then over the dorsal surface, especially anteriorly. The larval condition lasts during the first six weeks after hatching. Towards the end of this period the cement organ undergoes atrophy. The somatic gills atrophy later. During the process of their doing so, the *Lepidosiren* passes through a condition in which the stumps persist, evidently corresponding to that well known in the young *Protopterus*, the group of external gills with their common stalk having come to be situated

immediately above the fore limb. After the close of the larval period the *Lepidosiren* becomes much darker in colour and more lively in its movement. Young were obtained from the nest up to a length of 60 mm. About this time the cornea begins to assume the white unhealthy appearance that it has in the adult. In the young of this size small yellow spots appear, and in the young of 90 mm. these are conspicuous. Occasional yellow blotches persist in the young *Lepidosiren* of eighteen months, but in the adult they disappear.

The segmentation approaches most closely that of Ganoids. The shortening up of the invaginating groove is considered to illustrate a process which has taken place in phylogeny in the passage from the primitive holoblastic egg to the meroblastic condition. The continuity of the medullary folds behind the anus is adduced, together with the evidence of the prolongation of the blastopore along the floor of the medullary groove in other forms (Amphibia, *Ceratodus*), as affording potent evidence in favour of the hypothesis which derives the Vertebrata from ancestral forms as primitive as the Coelentera, and possessing an elongated mouth traversing the neural surface. The occurrence of external gills in the young of three so comparatively primitive groups of Vertebrata as Crossopterygians, Dipnoans, and Amphibians, their occurrence on four branchial arches in *Lepidosiren*, and on at least the hyoid arch in Crossopterygians, and the occurrence of a probable homologue on the mandibular arch in Urodela, are taken as suggesting that these structures are organs of great antiquity in the Vertebrate stock, and that there was formerly one present on each visceral arch. It is pointed out that were this so, it would afford a theory of the origin of vertebrate limb, which would be supported by much of the evidence brought forward by the supporters of Gegenbaur's view, and which at the same time would avoid the most important difficulties in the way of this theory.

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## The Man in the Beast.

Comparative psychology is the most anarchic department within the naturalist's province. The reasons for this are twofold. On the one hand, the subject-matter is in great part within the reach of all, and amateurs—most delightful people as long as they retain some sense of their limitations—rush in with inaccurate observations and incompetent inferences. On the other hand, the problems are intrinsically very difficult, so that it is at present hardly possible for the most careful experts to formulate even a small number of certainties to question which might be ruled irrelevant. Moreover, there are various vested emotional and other interests which are believed to be more imperilled by opinions regarding instinct than by those regarding protoplasm. All this being so, we are not assuming any position of superiority

when we say that the department of comparative psychology is notoriously anarchic, and a happy hunting-ground for the crank whose hand is against every man's. In this there is no reason for ill-humour, except that many people are led astray from the paths of clear-thinking, and that much energy is wasted in making bad inferences which might have been more profitably expended in making good observations. The only cure that we can suggest is that it might, without tyrannous censorship, be made, let us say, a point of etiquette, that a man should not publish his views without first making himself reasonably well acquainted with what has been done and said on the subject by a few of the leading representatives of the different schools.

These remarks are prompted by the perusal of a little book entitled "The Dawn of Reason,"[1] by Dr. James Weir, Jr., author of "The Psychical Correlation of Religious Emotion and Sexual Desire," etc.; and the particular characteristic of the book which seems to call for comment is its remarkable combination of good and bad qualities. It cannot be read without pleasure and profit, for it records in an entertaining way many very interesting observations; it cannot be read without disappointment and irritation, for its inferences are prejudiced and its prejudices are ignorant. Let us illustrate.

The author tells us in his preface that "his material has been thoroughly sifted, and the reader may depend upon the absolute truth of the evidence here presented." This is admirable; evidence whose absolute truth may be depended on is exactly what we need, for, as Dr. Weir reminds us, "many of the data used by the authors of more pretentious works are second-hand or hearsay." But before we get over the third page this author tells us that he is confident that, notwithstanding the fact that the nerve-*cell* is not differentiated in the primal forms known as the monera, or one-celled organisms [we are combining two sentences], nerve-elements are, nevertheless, present in them, and serve to direct and control life. We do not see the reason for the author's confidence, but we read that his "propositions have been formulated only after a twenty years' study of biology in all of its phases." We also read that all the other senses have been evolved from the sense of touch; that the visual picture formed by a crayfish's eye is a mosaic of hundreds of little pictured sections; that the author has been able to verify Professor Clark's observations in regard to the protozoan, *Stentor polymorphus*, which, as he asserts, has a well-developed nervous system; that on one occasion, while he was studying the contractile vesicle (heart) of one of these animalcules he saw it evince what seemed to him to be unquestionable evidences of conscious determination; and so on at frequent intervals through the whole book. We must believe the author when he tells us that "his material has been thoroughly sifted," but the meshes in his sieve must

[1] New York: Macmillan Company, 1889, pp. xiii. and 234. Price 5*s*.

be immensely large. After waving the standard of accuracy in the preface, the author might have refrained from citing in the body of his book data "given to me at a certain club banquet where I had no facilities for noting them down."

The author's loving enthusiasm for animal life, his desire to be generous in his appreciation of its psychical aspects, and his diligence in making personal observations and experiments command our admiration, and yet, except as an emotional protest against an extreme form of the theory of animal automatism, he has spoilt his work by the common error of mixing up observation and inference. This error is so insidious that no one, perhaps, escapes uninfected by it, but our author exposes himself to it wantonly. "Time and again," he says, "I have seen a snail draw in its horns when it perceived the white ball, to it an unknown and terror-inspiring object";—the inference that the ball was terror-inspiring is unproved. "Oysters kept in a reservoir and occasionally uncovered learn to keep their shells closed, . . . this is an act of intelligence." "I also saw a spider which intentionally beautified its web by affixing to it hundreds of minute flakes of logwood-dye." The honey-making ants "have evolved the idea of setting aside certain members of the colony as honey-makers." Even a sea-anemone has acquired the habit of "letisimulation" or death-feigning, for "I reached down my hand for it, when, presto! it shrivelled and shrunk like a flash into an unsightly green lump, and appeared nothing more than a moss-covered nodule of rock." "The champion death-feigner of the vegetable kingdom is a South American plant, *Mimosa pudica*," but in this case the author allows that the death-feigning is not due to conscious determination. He is convinced, however, by actual experimentation, that his fox-terrier falls into "brown-studies," "just as man does"; and asks if the dog may not then "claim one kind of abstraction." But one more example must suffice, for our entertainment is shadowed with a feeling of sadness. "I have seen my dog chase imaginary rats around my room after having been aroused while in the midst of a dream. His chagrin when he 'came to himself' and saw me laughing was always strikingly apparent." The author should seriously consider what Huxley meant when he said "the assertion that outstrips the evidence is not only a blunder, but a crime."

The value of the book except as a warning is in the author's personal observations—if only he had given them to us untainted by inference, but the point of the book is a protest against denying intelligence to animals. "Instinct," he says, "is the bugbear of psychology, and does more to retard investigation than any other factor. As long as people of the creationist stamp wield the instinct-club, just so long will they be unable to grasp the idea of intelligent ratiocination in the lower animals." We could understand this protest if it were true that modern workers in comparative psychology believed that all animals were, as Dr. Bethe holds in regard to ants and bees,

simple "reflex-machines," or automata, as Descartes suggested; but the facts are far otherwise, as such works as those of Professors Lloyd Morgan and Groos (to cite two familiar names) clearly show. The truth is that in avoiding an imaginary Scylla, the author has been caught in a real Charybdis of absurd anthropomorphism. He reads the man into the beast, and while his generosity touches our emotions, our fondness for animals, it leaves on our intelligence an impression of the grotesque. Let us illustrate "the man in the beast." "On one occasion I made some medusae tipsy, and their drunken gravity as they rolled and staggered through the water in pursuit of the light was as sorrowful as it was instructive; their actions in this respect were those of intoxicated men." He has been led to believe that "minute microscopic organisms have their pastimes and moments of simple amusement. . . . They seemed to be engaged in a game of tag." On another occasion he observed a meeting of two snails who "bowed several times in courtly salutation." And so on the story goes.

The author "does not claim infallibility"; he "*believes*, however, that future investigation will prove the verity of every proposition that is advanced in this book. These propositions have been formulated only after a twenty years' study of biology in all of its phases." We are sorry to be compelled to point out to an ingenuous public that the author does not really know what he is talking about. "The microscope shows," he says, "that these animals have notochords, nervous systems, and ganglia, or brains. With a one-sixteenth objective, and an achromatic light-condenser, I have been able to differentiate the gray matter in the brain of an ant, and even on two occasions to bring out the cells and filaments of the cortex." This *was* an achievement, but why drag in the notochord—a piece of skeleton! Dr. Weir believes that he is the first to claim the *sensual* importance of tinctumutation [colour-change], and the sense of direction or the "homing-sense"; he is also the first to claim that instinct is "heredity under a special name." He is also the author of the following statement of his position, which it would be unfair to omit:—"Ego-knowledge, while undoubtedly present in some of the higher animals, such as the dog, monkey, horse, cat, etc., is not a factor in the psychical make-up of any of the lower animals (insects, crustaceans, molluscs, etc.). But consciousness, so far as volition or choice is concerned, enters into the *psychos* of animals exceedingly low in the scale of animal life." With this we bid farewell to a work which is appropriately called "The Dawn of Reason."

# ORIGINAL COMMUNICATIONS.

## The Study of Natural History[1]

By J. Arthur Thomson, M.A., *Regius Professor of Natural History, University of Aberdeen.*

It has often been pointed out that the term "natural history" is in itself an index to the progress of science. Originally it was used to include the study of the whole outer world, human affairs alone excepted. At a later date it became equivalent to what is now called "natural science," the physical and chemical sciences having specialised off. More recently it became a synonym for zoology, as apart from botany, and this remains the most frequent usage. But there is an even more specific use of the term—for the study of the life of animals as it is lived in nature, for the study of their social and family life, their habits and inter-relations, in other words, as a synonym for what some would call "animal bionomics."

It will be admitted by all that the specialising of scientific inquiry suggested by the gradual narrowing of the term "natural history" has been as valuable as it has been inevitable, for there is no one now who either would or could take all nature for his province as men like Buffon did. At the same time, it may be allowed, even by those who fully recognise the value of specialism, that the old term "natural history" strikes two notes which both teachers and students are nowadays occasionally in danger of losing.

### *The Unity of Science.*

The first note, if I have heard it aright, suggests the unity of science. For, after all, there is but one great subject of concrete study —the order of nature; and, although we split up the study of this into sciences and sub-sciences, we are simply making an adaptation to our limited intelligence — an adaptation which inevitably becomes fallacious if it lead us to forget that, without co-operation and correlation

[1] An Introductory Address delivered at the University of Aberdeen.

of results, no permanent steps of real moment are likely to be made. This note of the unity of the sciences is sounded whenever we say the word university. Its value is demonstrated by the history of the sciences, which shows how often a fresh contact between two departments—chemistry and biology, biology and psychology—has led to great advances. It is also demonstrated when we consider a big subject, like the physiology of marine organisms, which there is no hope of understanding except through the combined efforts of chemist and physicist, botanist and zoologist. How hopeless it would be for the unaided biologist to suggest an answer to the recent question—Why is the population of swimming and drifting marine organisms denser in the arctic than in tropical or sub-tropical seas? Whether we take a hint from the way in which Linnaeus used the term "Organisata" to include both animals and plants, or from the title "Animated Nature" which the keen-witted amateur Goldsmith used, or from Comte's hierarchy of the sciences, or from the late Principal Caird's essay on the unity of science — I have purposely chosen incongruous examples—we hear the same note of unity towards which our teaching and learning must move, even if the curve be asymptotic. The study of living creatures stands midway between the chemical and physical sciences, which are in a sense beneath it, and the mental and social sciences, which are in a sense above it; there are lights from beneath and lights from above; and to attempt to shut out either means obscurity. It is evident, for instance, to every one who has considered the matter with an unbiassed mind, that what are called the evidences of organic evolution owe much of their cogency to corroborations from other sciences than biology.

### *The Life of Living Creatures.*

The second note which seems to be sounded by the old naturalists who clung to the term "natural history"—men like Gilbert White, for instance—suggests that our first and our final problem is with the life of living creatures in a state of nature. Amid the undoubted and surely legitimate fascinations of dissection and osteology, of section-cutting and histology, of physiological chemistry and physiological physics, of embryology and fossil-hunting, and the like, do we not need to be reminded sometimes that the chief end of our study is a better understanding of living creatures in their natural surroundings? In what I may perhaps call the keenly analytic atmosphere of modern zoology there is no risk that any one will fail to appreciate the value of the abstractions which we make when we study the dead beast on the dissecting tray, or sections of parts of it under the microscope, or solutions of parts of it in the test-tube—but is there not a risk lest we forget that the picture and the problem with which the naturalist starts, and to which he must eventually return, is the picture and the

problem of the shore-pool, the wayside pond, the open sea, the moorland tarn, the bird-berg, in short, wild nature and its drama of life? Unless we get back to that we are forgetting the picture and the problem with which we start, the outlook which our eyes afford us—in other words, the facts of the case. It is this second note which I wish to catch more precisely. To show its strength and clearness, and that we may be encouraged in listening for it, I have ventured on a short statement of the problems of the naturalist, freed as far as possible from personal bias, a programme, as it were, which would be admitted by all to be in no way one-sided and yet so disposed that the value of the old naturalist's note may be made evident. I wish to suggest that natural history or bionomics cannot be either stable or progressive if it stand aloof from the results of the anatomical and physiological, embryological and palaeontological disciplines; and on the other hand, that these disciplines are to be saved from losing perspective and interest by being kept in touch with life as it is lived in nature.

### *The First Question—What is This?*

The naturalist's first question—however learnedly he may phrase it—is one of the child's first questions, asked long before it can speak —"What is this?" In how many different tones—of fear, of awe, of wonder, of inquisitiveness—has this question been asked since man and science began! Was it not Aristotle's question when a new specimen was brought to him? was it not the question on the *Challenger*, when the dredge came up? is it not the question on the lips of every teacher and student of natural history to-day?—What is this? It is a "simple question," but how hard to answer, as we press it further and further home, from external features to internal structure, from organs to tissues, from tissues to cells, as we put one lens after another in front of our own, as we call to our aid all sorts of devices —scalpel and forceps, razor and microtome, fixative and stain. "What is this," we say, "in itself and in all its parts? what is this by itself and when compared with its fellows and kindred?" and our answer broadens and deepens till it furnishes the raw materials of the science of Morphology. Inquisitiveness is one of the prime movers of science, and one of the diagnostic characters of the scientific mood is a dislike of obscurities, of blurred vision, and of fogginess. The student who is never sure whether he sees a thing or not, who is satisfied with the ill-defined image seen through an imperfectly-focussed microscope, whose dissection is invariably like a jungle, has not the scientific rudiment in him. We must have the ideal of seeing clearly. The mole has a sort of half-finished lens, which is physically incapable of throwing a precise image on the retina. If there is any image it must be a blurred tangle of lines. To press home the naturalist's first

question—What is this?—is pre-eminently a discipline which tends to save us from having moles' lenses.

But, to be quite frank, there are few who are able to acquire the power of precise vision except within a restricted range; and even within a limited circumference the discipline is not easy. Many can sympathise with one who studied under Louis Agassiz, and relates that she was almost brought to despair by the way in which the master, having given her a specimen, came day after day, and asked her with a severe kindliness, "Well, what do you see now?" But at length she saw something—saw what was to be seen.

Those who have a strong interest in the life of animals as it is lived in nature are often keen and precise observers, but it is not always so, and the field naturalist often loses both satisfaction and effectiveness by failing to appreciate the depth of the question, "What is this?" It is idle to theorise about flowers and insects unless one is prepared to make observations in regard to particular flowers and particular insects—which raises the problem of species-identification. It is absurd to suppose that a precise terminology, which no one complains of on board a yacht, will not be even more necessary in the complex system of nature. It simply will not do to see the bird merely as "a drift of the air brought into form by plumes"; one must see it—see it really—as Kipling sees the locomotive or the steamship, and grasp something of its essence by a full knowledge of its anatomical inwardness. Cuvier discouraged, but eventually saved a student, who thought he had made a new discovery, by asking him if he had ever dissected an insect, and by bidding him do so.

In short, all experience goes to show that there can be no sound natural history without the anatomical, morphological, precisely analytic discipline which gives a partial answer to the question—"What is this?" The greatest of naturalists was a classifier and anatomist of the barnacles. On the other hand, I wish to suggest that we need the note of natural history to give interest and sanity to our morphology. What is the justification of the enormous amount of energy expended every year in adding to the mass of results reached by morphological analysis, and in mastering some small fraction of it for oneself? No one can pretend to have even a literary acquaintance with the whole vast volume of morphological facts. That is more impossible than it is for a man to know, or even to have seen, the whole of London. What we wish rather is to be able to read the volume, to find our way about, as it were, and to be able to add to the volume too when occasion demands. It is difficult to see any justification for merely adding to the volume aimlessly; there seems rather every reason to abstain from so doing. What we want is certainly not an encyclopaedic morphological knowledge. That is impossible without too great a sacrifice. We seek rather a general understanding of the chief styles and laws of

organic architecture, a general view of the certainties and possibilities of blood-relationship among animals, a realisation of what the word "organisation" means, and one criterion which will keep both teaching and learning sane and interesting is the old criterion of natural history. We must ask, Does this anatomical detail help us to a deeper and clearer insight into the real life as it is lived in nature?

There are other criteria, which I do not propose now to discuss, such as the rather dangerous criterion of practical utility, and what may be called the biological criterion which asks whether this or that piece of research or knowledge will help towards an understanding of the big generalisations of the subject.

### *The Second Question—How does this Act?*

But close upon the first question—What is this? there rises a second—How does this act? It is equally natural and necessary, and throughout the progressive periods in the history of biology the two questions have never been far apart. They have evolved together, especially during this wonderful century, prompting one another to a more and more penetrating inquisitiveness. The key-word of the one is structure or organisation, of the other function or activity. The creature which our first question killed and picked to pieces has to be put together again and made to work. What does it do? how does it do it? how does it go? how does it keep agoing? how does it set other creatures like itself agoing? how long can it go? how does it cease from going? In other words, how does the organism feel and move? how does it grow and multiply? how does it waste, recover itself, and finally, in most cases, die? Above all, what is the secret of its activity and of its power of effective response to the order of nature? These are some of the physiological questions which recall Clerk Maxwell's boyish question—What is the go of this—the particular go of this?

We brush aside the old answers which speak of vital spirits and vital force, of conflicting temperaments and principles of life, not forgetting that we must sometime rehabilitate the truth which is in these verbalisms; we analyse the workings of the different organs which co-operate in the life of the whole, such as the beating heart and the digesting stomach; we pass beneath this to study the tissues that build up the organs—the contracting muscles, the controlling nerves, the secreting glands; we inquire yet more deeply into the waxing and waning of the cells and the complex chemical changes which occur in these; and we find that our second question is as unending as the first.

Here and there we make clear a chain of events, a procession of causes; we discover the hidden uses of various parts; we begin to understand certain corners of the internal economy; but to counteract any feeling of self-satisfaction we have simply to watch

any animal living its life in nature, responding effectively to its environment, and to ask ourselves again—How does this act? We must then confess that the way of the eagle of the air is still too wonderful for us.

The term physiology is now for the most part restricted to a study of the individual organism, and especially to a study of its internal economy. Just as anatomical analysis may be compared to picking a watch to pieces—an operation which dimly suggests the delights of dissection—so physiological analysis may be compared to a study of the kinetic aspect of the watch, and even when physiology becomes comparative it is still like comparing one kind of watch with another. To save the results from inexcusable partiality and incompleteness it is necessary to sound the natural history note, the recognition of organisms in the plural, as members of a pair, a family, a flock, an association, a fauna, as threads in a web of life, as agents in a complex environment. In our everyday work in the laboratory we have the bad habit of speaking of the earthworm, the crayfish, the skate, but to the naturalist these words are all in the plural.

It is recorded that young hermit-crabs which had been reared from eggs in an aquarium, and had never seen a shell, at once seized some shells which were dropped into the water, and had begun to explore them before they had reached the bottom. It is idle to say that this effective response is not as essentially part of the nature of the hermit-crab as its gills or gizzard. For the physiologist to ignore the answer which the hermit-crabs made to the stimulus of the shells would be as illegitimate as to ignore their respiration and nutrition. In short, no one can seriously maintain that physiological analysis has only to do with cells, tissues, and organs; it has equally to do with the intact living creature in its natural surroundings, with its domestic and social relations, with its habits and adaptations, with its struggle for existence and endeavour after wellbeing. Physiological analysis thus completes and corrects itself in "Natural History."

It is evident that physiological study supplements the anatomical answer to the question—"What?" The latter, if we are patient, will make a living creature practically translucent, will enable us, without any Röntgen rays, and, with our eyes shut, to see the animal through and through, each organ in its place. But the physiological discipline, if we are patient, will give us more—will give us a working thought-model, an intellectual cinematograph. I do not mean to suggest that there are not other results of anatomical and physiological study besides an increase in our power of visualising; but that is one result, and a very important one, for clear seeing is often the first step to clearer understanding.

We have heard wise warnings against the extremely analytic method of modern biology, against the necrology which is always destroying in the effort to understand. I have already recognised the danger in-

volved in the fact that our methods force us to abstract certain aspects of the organism. There is an undoubted risk lest we forget the unity of the organism which we take so carefully to bits; there is an undoubted risk lest we forget that what we measure and weigh and analyse belonged to a creature which had something analogous to our personality. We cannot dispense with our analysis, however, and the corrective for its partiality which I am at present pleading for is simply more study of the real life of the creature in its natural environment, in other words, more "Natural History." It will then be found that in proportion as our analysis has been thorough, our realisation of the life around us will be vivid. To say that no one really knows a bird who has not watched it build its nest may be true; but it may be justly retorted that no one really knows a bird who does not know something about the physiology of its respiratory system.

*The Third Question—Whence is This?*

A third question is—"Whence is this?" and though it is probably as ancient as the others, the answering of it is distinctly modern. It is really a double question, for we may inquire into the development—the becoming—of the individual, and we may inquire into the history of the race to which the individual belongs. We may study the child-animal in its cradle—the bee-grub in the comb, the embryo skate in its mermaid's purse, the chick within the shell—and the answer to the question—Whence came this individual animal as a whole and in each of its parts? is embryology. On the other hand, we may study the history of the race as it is hidden in the graveyards of the past, the fossil-bearing rocks, and the answer to the question—Whence came this race? is palaeontology.

To suggest how a minute embryological detail may become full of interest to the field-naturalist, I may refer to the familiar jelly which surrounds the eggs of the frog as these lie in the pond or ditch. The jelly is an albuminoid secretion formed by the walls of the oviduct round each ovum, and obviously comparable to the white in a bird's egg, though it does not seem to have the nutritive importance of the latter. So far a bald and not very interesting statement, but let us leave the spawn in the pond and consider the uses of the jelly there. It increases the buoyancy of the floating eggs; it renders fertilisation more secure by keeping the eggs together as they are laid; each sphere serves as an elastic cushion which lessens the risk of breakage to egg and embryo; the material is so slippery that few birds, except those with broad bills, can manage to swallow it; the stuff is unpalatable, so that even the voracious Gammarids leave it alone; and it has also been suggested that each crystalline sphere may serve as a sort of glass-house or incubator, enabling the developing egg to make the best of the sun's rays. On count after count even a dead

detail of structure, such as the jelly of a frog's egg, is justified by its utility.

Walt Whitman has it that "the tree-toad is a *chef-d'œuvre* for the highest," but perhaps it is not necessary to go beyond the spawn to learn the lesson taught to the apostolic fisherman that there is nothing common or unclean. Unless we soon learn that, there must be something seriously wrong with our learning or with our teaching.

Much of embryology is simply the anatomy of early stages, a little of it is an attempt to understand the physiology of growth and differentiation, and both these inquiries are obviously necessary to an understanding of the organism, but it is not difficult to show that embryology will be robbed of no small part of its interest and meaning if the natural history note—the bionomical note—is not sounded. The organism does not develop *in vacuo*, but in an actual environment, often a very definite one, which supplies stimuli of undeniable importance in liberating the developmental potencies, and imposes conditions to which many of the details of development are evidently adaptations.

Similarly, a true appreciation of palaeontology must also recognise the natural history note. For, although from one aspect palaeontology only differs from anatomy in that the subjects of examination died millions of years ago and are very badly preserved, from another aspect palaeontology is the study of racial history. To speak of the palaeontologist as a Dryasdust who pores over antediluvian vestiges, as an anatomist under difficulties, as a fossil-hunter, as a burrower in graveyards, is a caricature. The palaeontologist is the historian of the times before history, the specialist on the rise and fall of races, the student of "Werden and Vergehen," the biological scholar who endeavours to interpret the present in the light of the past. Now, just as it would be an imperfect archaeology which simply described remains without reviving in imagination the ancient life, so it is an imperfect palaeontology which does not, as cautiously as you will, at least try to reconstruct not the skeleton only but the living creature and the drama of life in which it played its part.

On the other hand, we may again to advantage sound the natural history note in recognising that what, after all, we especially require of palaeontology is to give us a better understanding of the life of the present. One step was taken by Cuvier, who insisted on ranking the fossils along with the modern types in a unified zoological system. Another step was taken, hesitatingly, by Louis Agassiz, who, though no evolutionist, recognised that "the history of the individual is but the epitomised history of the race"; and a further step, which may be called evolutionist, was taken when it was recognised that the present is the child of the past, though we do not yet understand how it was brought forth. These three steps have led us to feel that if we are to understand the life of the present in its distribution, its relation-

ships, its complex inter-adaptations, and its strange turns and twists of individual development, we must look for light in the inconceivably distant past. Just as we may see to read by the fireside of to-day by a light which is obviously the transformed sunshine of the carboniferous era, so the animal life of the past illumines that of the present, and it is on this account chiefly that we study it. To take a concrete instance, the migration of birds is a fascinating and extremely puzzling problem in natural history, the solution of which is still far off; but it will be admitted by most that the facts have become in certain aspects more intelligible by being considered in connection with the evolution of climates and with the great Ice Age in particular.

### *The Fourth Question.*

There remains a fourth question, also ancient, but now asked with a new hopefulness—"How have these living creatures come to be as they are?" They have had a history—a slow racial evolution—as surely as they have an individual development; what have been the factors in this history? How has the raw material of progress, which we call variation, been made available throughout the countless ages? and how has this raw material been fashioned to shape and use? We have got a firm grasp of a modal interpretation—that the present is the child of the past—but we must beware of confusing this with a causal theory. The idea of evolution is the most potent thought-economising formula which the world has yet known, but as to the factors in the evolution we are still only inquiring.

I have said that there cannot now be a stable and progressive natural history which does not ask the three questions—What is this? How does it act? Whence has it come?—which does not, in other words, recognise the methods and results of morphology, physiology, embryology, and palaeontology; but I do not say that all useful natural history work must have this completeness of inquisitiveness or outlook. History would contradict this, though I doubt whether any of the immortals on the roll-call of naturalists did not in his fashion ask all the questions. It is plain, however, that Reaumur was little concerned about the palaeontology of insects, and that Gilbert White knew nothing of the embryology of the earthworm. It is plain that Christian Konrad Sprengel was not much interested in histology, and that Buffon knew little of the physiology of the individual. Yet all the four were such splendid naturalists that it seems almost an impertinence to praise them. Just as I presume no one would deny the artistic mood to the author of a picture which was exquisite in form but weak in colour, or of another which was feeble in its form but a joy for ever in its colour, so we reverence the old naturalists none the less in admitting their limitations.

This saving paragraph is necessitated by the fact—the historical

fact—that there have been many consummate naturalists who never asked the fourth question at all, who took nature as they found it, saw it, saw its activity, saw its "whence," but never inquired as to its "how"! To omit this question now would be to label oneself a Rip Van Winkle; not to have asked it in pre-Darwinian days, or to have refused its legitimacy as a scientific question was quite congruent with the intellectual environment of the time. But Darwin gave naturalists a new organon, not to use which means "an unlit lamp and an ungirt loin."

On the other hand, it is equally evident that in our attempt to answer the question "How has this come to be as it is?" we must keep in touch with the actual facts of the case. We may take advantage of intricate mathematical methods in our study of variation, but we must be continually returning to the actual cabbages in the garden and the real herrings in the sea. We may take advantage of logical controversies about natural selection to sharpen our intellectual weapons, but we must be continually returning to an actual observation and measurement of the struggle for existence among the crabs on the shore or among the seedlings in the plot. We may use abstract hypotheses, such as Weismann's theory of the germ-plasm—which even its fiercest critics must admit to be one of the most logically ingenious and complete hypotheses in the history of science, and to have provoked research more than any other biological idea except that of natural selection—but as Weismann says—"the test of the usefulness of theories lies in the applying them to special phenomena; it then becomes apparent whether and where they touch improbabilities, and where new facts are required to improve them or to replace them by others." We may think out our hypotheses by the fireside, but this will be of little avail unless we condescend to test them in open air.

Even in regard to experiment, while there is no lack at present of ingenuity in playing tricks with cells and eggs, embryos and larvae, and while no one can predict the outcome of what Darwin called fools' experiments, it is plain that those are likely to be of most value which are definitely related to the possibilities of actual occurrence in nature.

It must be clearly understood that the evolutionist has not at present any rounded-off and demonstrated generalisations like the law of the conservation of energy or the law of gravitation. He has a certain way of looking at things, he believes that they have had a natural history, he has a formula, a modal interpretation, the general idea of evolution. He has also particular causal theories, all of them very young, which he seeks to test, but which from the nature of the case he has little hope of demonstrating in the strict sense. With his general idea and his particular causal theories, his continual occupation is to see whether his keys fit the locks of actual fact—an occupation of perennial interest. Sometimes he has fancied himself

the possessor of a master-key, but some new lock has baffled him, and it seems likely that this will be his experience for many years to come. In one of his essays, Professor Weismann rejects the common opinion which compares science to a building resting on sure foundations and rising from a firm basement safely to the highest story; on the contrary, he says of the exact sciences—"They all build from above, and not one of them has found a basement yet—not even physics."

### *An Illustration.*

Let us now consider a single fact of natural history so that we may see more clearly how an understanding of it makes demands on all the resources of our discipline. It is well known that the mole stores earthworms for use in winter—as many as a hundred being sometimes found together; it is a less familiar fact that these stored earthworms are, in many cases at least, decapitated, so that although alive they are unable to crawl away. We may begin by asking how the mole and the earthworm happen to be both underground, which is certainly not the primitive home of their respective races; we pass on to inquire how the mole catches the earthworms, and how they are often able to avoid the mole; another query raises the problem of the storing instinct; a side question leads us to consider the mole's imperfect lenses and the peculiarity of its optic nerves; then we are bound to inquire how it is that the decapitation of the earthworm prevents its crawling away; and this brings us to consider the function of the head-ganglia as a motor centre. We know that in summer a lost head-end may be re-grown, but that this does not occur in the low temperature of winter, a fact that raises the whole difficult problem of regeneration; we know that the re-grown anterior part sometimes turns out to be a second tail instead of a new head, which forces us to face the fact that there are limits to the perfectness of adaptations. We cannot avoid inquiring, too, how far the mole's destruction of earthworms, which are for the most part valuable agriculturally, is compensated for by its destruction of injurious insects. And so from the unearthed store of earthworms in the mole's burrow we pass to problems of nerve physiology, comparative psychology, conditions of development, rural economy, and what not—inquiries which surely become more real and interesting to investigator and student alike when they are centred round an everyday fact of natural history.

### *The Essence of Natural History.*

When in our anatomical or zootomical studies we propose to dissect the skate or the snail, or the like, we must first isolate it from its environment and kill it; in other words, we have to make an abstraction—a very necessary and valuable one, but still an abstraction.

And just as no one dreams that the study of man begins or ends in the dissecting-room, so we must supplement our comparative anatomy, physiology, and embryology of animals by getting back to the natural history picture. If we do this carefully we shall probably discover that our study of organisms is even wider and grander than we had supposed. We see the living creatures as parts of a great system of things, amid which they live as unities, especially marked off from things which do not live by their characteristic power of effective response, and bound to other components of the system by complex inter-relations, to appreciate which fully is indeed beyond the reach of any one man, since it means a grasp of natural history in the oldest, widest sense of the term—the science of the order of nature. Thus, the two notes suggested by the old term "natural history" are really one—the actual life of organisms implies such complex and far-reaching inter-relations that we need all science to enable us to appreciate it.

We speak of the correlation of organs—meaning that the various members are closely bound together in one body—but we do not speak enough of the correlation of organisms. Whether we take Gilbert White as representing the old school, or Darwin as representing the new, we get the same impression of nature as a vibrating system most surely and subtly interconnected. And to have the vision of the web of life is one of the ends of our study. We are familiar with striking instances—the links between cats and clover crop, between earthworms and the world's bread supply, between white ants and tropical agriculture, between ivory ornaments and the slave trade, between bicycles and the persecution of the walrus, between stable manure and the carp in the fish-pond—all "house-that-Jack-built" conundrums until the procession of causes is worked out. But what we have to realise is that these curiosities are simply quaint examples of what the naturalist sees or has to find throughout the world. One may say, with all reverence, that much of Darwin's work has been an eloquent commentary on that memorable saying regarding the sparrow that falls to the ground. Such a simple event literally sends a throb through nature; we can follow its effects a few steps, just as we can follow for a few yards the ripples made when we throw a stone into a still sea; in neither case can we doubt that the spreading influences are real though they pass beyond our ken.

### *In Conclusion.*

Perhaps, in conclusion, I may suggest another advantage in continually returning to the facts of the case, to living creatures as they live in nature. This other advantage is that it saves us from the common error that the naturalist has arrived at any ultimate explanations of things, or that as naturalist he even seeks for them. The word "ultimate" does not occur in the scientific dictionary. Science

describes, analyses, discloses chains of sequence, gives mediate explanations, perhaps, but certainly no ultimate explanations. We get hold of certain fractions of reality which interest us, we seek to see them as clearly as may be, to put them in an ordered series, to reduce them, if possible, to a common denominator, but we do not explain our common denominator, any more than the philosopher his greatest common measure. We may seek to explain certain facts in terms of protoplasm, but we do not explain protoplasm; we continually speak of the organism, but no one knows what the secret of its unity is; we believe that animals have been "evolved," but we know that to say so is to raise a problem rather than to solve it. From the careful study of any biological question we are bound to end in this recognition of the relativity of our aims, to feel, as Fouillée says, that science is like a broken mirror, whose reflections of reality philosophy seeks to reunite, but if at any time we should be tempted to ignore it in our satisfaction at the illumination of some dark corner, or at the successful testing of some hypothesis, we shall at once regain sanity if we go out into the midst of the real life of sea and river, of wood and meadow, and looking Nature full in the face dare to say—"We can explain all this."

I have spoken of the naturalist's work only in so far as it helps us to a greater appreciation and understanding of the life of animals in nature. I have said nothing of its relations to the practical life of man which animal life touches at so many points. I have said very little of its relations to the big problems of biology—protoplasm and the cell, growth and reproduction, heredity, and the like. To have discussed these would have obscured my simple thesis, which is just this, that a stable and progressive natural history requires all the help which the special disciplines—the answers to our four questions—can afford; and, on the other hand, that the special disciplines lose in interest, dignity, logical completeness, and effectiveness if they lose touch with natural history.

A well-known philosopher is reported to have spoken of natural history as "one of those Kindergarten subjects," but perhaps there is no reason for us to take offence at that. We do not go far before we find that the study of natural history raises problems even to state which requires all the intellectual virtue and manliness which we may have; and yet the suggestion of the Kindergarten prompts me to say that we shall not do ill if we can study our subject with the inquisitiveness, open-mindedness, daring, and humility of children. For it is true of the kingdom of science, as of the Kingdom of Heaven, that it is as little children—cautiously trying our steps—that we may enter in.

# Fresh-Water Biological Stations: America's Example.

By D. J. Scourfield.

About two years ago Prof. Anton Fritsch, the veteran Bohemian naturalist, called the attention of biologists in this country (*Nat. Sci.* March 1897, p. 169) to some of the work being done on the Continent in connection with the establishment of fresh-water biological stations. It is now proposed to supplement Prof. Fritsch's article, and thereby of course to enforce its moral that England is altogether behind the times in the matter of fresh-water biological stations, by giving a few details of the progress made in America in this respect.

It was in 1893 that the first serious attempt was made in the United States to undertake detailed fresh-water biological work on modern lines. In the summer of that year a small party of enthusiastic naturalists, under the able leadership of Prof. J. E. Reighard, and supported by the Michigan Fish Commission, established a temporary biological station on Lake St. Clair. The work of the party was directed towards the elucidation of the biology of the lake in all its phases, with the practical end in view of discovering the actual facts concerning the environment of the white fish, the yield of which in the great lakes had been steadily diminishing, in spite of a large outlay for artificial propagation. The outcome of the two months' work, although not leading at once to any very definite practical result, was very valuable from a scientific point of view, as may be seen by a reference to the papers on the plankton and the different groups of the fauna and flora of the lake published in the *Bulletin of the Michigan Fish Commission* (see Nos. 8, 13, 16, and 17 of list appended).

The next year (1894) saw a very considerable increase in the amount of attention paid to fresh-water biology in America. The enlightened Michigan Fish Commission again enabled a temporary biological station to be maintained; on this occasion at Charlevoix, for the investigation of Lake Michigan. Excellent work was accomplished here by Prof. H. B. Ward and his companions, and a summary

of the principal results obtained was published in No. 6 of the *Bulletin* already referred to (23).[1]

During the same year, Prof. C. D. Marsh commenced (following up some preliminary work of the previous year), his systematic investigations on the limnetic crustacea of Green Lake in Wisconsin (14). Prof. E. A. Birge also started his elaborate "Plankton Studies" on Lake Mendota in the same State (1). Although in these cases the observations were not carried out by means of a formal biological station, the work done has been essentially such as would occupy such a station, and in fact the position of the University of Wisconsin on the shore of Lake Mendota enabled Prof. Birge to have all the advantages of a biological station without the trouble of specially organising one for his research. The important results of Prof. Birge's work have been already alluded to in this Journal. (See *Nat. Sci.* vol. xii. pp. 295-6.)

But the most notable event of the year 1894 was the establishment, by the Illinois State Laboratory of Natural History, in conjunction with the University of Illinois, of a biological station at Havana on the Illinois River, under the direction of Prof. S. A. Forbes. This station took the form of a large floating laboratory, affording working accommodation for about twenty persons. It was specially built for the purpose it has to fulfil at a cost of $1,255, and it has been equipped with all kinds of collecting and other appliances used in biological investigations. As regards literature, the station is able to draw upon the library of the State Laboratory of Natural History, which is exceedingly well supplied with works on fresh-water animals and plants. Besides the floating laboratory, a small steam-launch also forms part of the station equipment, together with a number of row-boats (4 and 11).

The district worked by the Illinois station, viz. the Illinois River near Havana, and the lakes, ponds, and swamps immediately connected therewith, appears to be an extremely interesting one from the biological point of view, and a considerable amount of valuable investigation has already been carried out by the station staff and visiting naturalists. The general object has been to study the forms of life, both animal and vegetable, in all their stages, of a great river system. It would be absolutely impossible, in an article of this kind, to allude in detail to all the results which have followed from the establishment of the station. Some have been noted from time to time in *Natural Science*, but a few indications may also be given here of the kind of work that has been done.

From the very first, quantitative collections of the plankton organisms have been made at regular intervals from certain selected

[1] A biological survey of Lake Erie, supplementing the work done on Michigan and St. Clair, was undertaken last year under the auspices of the U.S. Commission of Fish and Fisheries, but no results have yet been published.

localities representing different types of environment. These were at first made by a modification of the Hensen-Apstein methods, but as the work proceeded several important alterations were introduced, such as the employment of a pump for the collection of the plankton material, and of a centrifuge for its rapid deposition. Numerous painstaking experiments have also been made with a view to determine and correct the errors in quantitative plankton methods in general, and all workers in this field are indebted to Dr. C. A. Kofoid, who has had charge of this department, for the light which he has thrown on the subject (9 and 10). Unfortunately, owing to the large amount of labour involved in working out the collections, and the small staff available for the purpose, none of the results of the actual work on the plankton have yet been published. When they do appear, however, they can hardly fail to be of the highest scientific value.

The aquatic insects have received a large share of attention at the Illinois Biological Station, and it has been the special endeavour of those working at these forms to get full particulars of the life-histories of the species, both by constant collecting and by breeding. In this way a large amount of information has been collected on the immature stages of over two hundred different species, the great majority of which had not been previously known (5).

Besides the insects many other groups of animals have been carefully and profitably studied, as is abundantly clear from actual publications dealing with the Protozoa and Rotifera (6 and 7), Oligochaeta (21 and 22), Ostracoda (20), and Copepoda (3, 18 and 19). Materials have also been accumulated for treatises on several other important groups.

In addition to the special researches carried on by the station staff and visitors, an attempt was made last year to run a summer school of biology in connection with the station, with regular courses in botany and zoology. Although hardly so well attended as was anticipated, the scheme was thoroughly successful (4, 1897-8). Such a school should be of the greatest practical importance in the training of biological students, and it is to be hoped that when the exceptional advantages offered by the Illinois Biological Station are better known there will be no need to complain on the score of attendance.

The year after the establishment of the Illinois station, *i.e.*, in 1895, Professor C. H. Eigenmann was enabled, with the aid of the Indiana University, to open a biological station on the shores of Turkey Lake in Indiana. The main scientific object of this station has been the study of variation, and to this end a small lake, within which the influences of environment are presumably reduced to a minimum, was purposely selected. During the four years since the starting of the station a large amount of material has been obtained to illustrate such points as the annual variation, the birth mean, and the effect of selective destruction, but only a portion of this has yet been thoroughly

examined. Among other published results, however, has been the issue of a valuable paper by W. J. Mönkhaus on the comparative variation of two species of darter fishes living in Turkey and Tippecanoe lakes (15), from which it appears that the specimens of both species in one lake differ in every structure examined from those in the other; further, that they are modified in the same way in the the same lake, and that this is not the result of selective influence but apparently the direct effect of the environment. Apart from the scientific work accomplished by its aid, the biological station has also been of immense service to the Indiana University in connection with the training of biological students, of whom no less than 105 made use of the station last summer.

A year later (1896) yet another fresh-water biological station came into existence in the United States. This was the Lake Laboratory of the Ohio State University, established at Sandusky on Lake Erie, under the direction of Professor D. S. Kellicott. This station was started mainly to afford favourable opportunities for the students and instructors of the university to study the fauna and flora of one of the Great Lakes and of the rich zoological district around Sandusky. The subjects to which special attention has been given during the time that the station has been open, have been the fishes, their food and parasites, the aquatic insects, the rotifers, the sponges, and the crayfishes. Some progress is also said to have been made in the investigation of the plankton. As nothing seems to have been published, however, bearing directly on the results obtained by the workers at the station, it is not possible to give any details on this subject. It is sincerely to be hoped, nevertheless, that, notwithstanding the death of Professor Kellicott, the work of the station will be continued and its results given to science in due course.

In addition to the foregoing stations actually in existence, proposals have been made to establish similar institutions in Iowa, at Rochester in western New York, and elsewhere. Canada, too, is hankering after a fresh-water biological station, although in spite of the grant of £75 by the British Association when in Toronto, nothing definite appears to have been done in the matter. Altogether there appears to be little doubt that the year 1900 will see at least half a dozen fresh-water biological stations at work in America.

The contrast between such a state of affairs and that existing in this country is surely enough to make us ask anew,—When will our biologists wake up to the fact that good systematic work can be done and important biological problems attacked by fresh-water as well as marine stations? When shall we be able to report to our Continental and American confrères that, in the words of Dr. Fritsch, England has done her duty by fresh-water biology? Unfortunately it is only too true that up to the present **England has not done her duty** in this matter, and as a striking instance of this it may be pointed out that of

the very considerable knowledge which we now possess about the organisms constituting the plankton of fresh-water lakes—*e.g.* their remarkable and often complicated variations in abundance during the year, both collectively and taken species by species, their vertical distribution in regard to the season, to the time of day, to the so-called "Sprungschicht," their occasional seasonal variation of form, their relation to one another and to the other lake forms, notably the fishes, —of all this and much more not one single fact has been contributed by this country! Who said "Shame"?

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# Weismann's "Regeneration."

By Professor Marcus Hartog, D.Sc.

*Forewords.*

The following abstract of certain principles of controversy implied in Prof. August Weismann's paper on "Regeneration" in the April number of *Natural Science*, is not intended to be of the nature of "invectives, sarcasm, or derision" (p. 323); for these weapons, the matter is far too serious. The human love for a complete system is so great that any clearly expounded system that covers a very large number of well-known facts is sure to be widely adopted, so long as those other facts that are incompatible with that system are unknown or ignored. Weismann's own contributions to our knowledge of facts are so numerous, careful, exact, and important, his earlier theoretical work is so valuable in every way, that his name added weight to his "Systema Generationis" even in its earlier, recalled editions. His candour in admitting errors therein, when they had been demonstrated to the world, his unremitting patience and ingenuity in rebuilding his edifice after such a ruin as befel its foundations in his papers on the meaning of sexual reproduction, are deserving of our warmest admiration and sympathy. We owe him a great and enduring debt for clearing our ideas, for making us give reasons for our faith, for banishing many an old error from science. In his last paper, however, of which he says, "I have written it rather for the supporters than for the opponents of my views," he has pursued a course fraught with dangers, masked by the greatness of the authority of his name, and only to be averted by being brought clearly into light. The canons of argument and ethic which present these dangers are therefore summarised, with quotations from his text in which they are exemplified; and this also is done for the benefit of those for whom the paper is written—"the supporters rather than the opponents" of the germ plasm hypothesis. For their benefit again it is desirable to point out that "Auslösung" is used by Hertwig in its current technical physiological sense as equivalent to "discharge"—of gunpowder, not of a cargo-boat; the connoted "setting free" is a setting free of energy, uot of matter; and, consequently, its translation by "liberation" in this paper is incorrect,

and carries with it a fallacious and question-begging connotation of the "liberation" of Weismannic determinants.[1]

The debt all thinkers in biology owe to Weismann makes the writing of this abstract an ungrateful, if necessary, task. To go further and to criticise is fortunately unnecessary.

*Canons.*

*Canon I.—If* ONE *of many possible hypothetical explanations of a process is refuted, Weismann's explanation of that process is* PROVED.

*Example.*—"In the face of such facts the hypothesis of a general power of regeneration, which restores the life crystal to its integrity, is obviously untenable. Here the crystal is restored after an older pattern, and one cannot see why this should be so, since the whole has been no more altered than a dodecahedral crystal would be if the point were broken off. If in that case the missing portion were suddenly to be recrystallised on another 'system,' it could only be because the elements from which the restoration started were not the remaining whole but particular parts belonging to another system, or —to translate into the language of biology [*this is a synonym for expression in terms* of the germ plasm hypothesis]—because at the injured place there were regeneration 'Anlagen' of earlier origin, which came into play as the result of the injury. The existence of 'Anlagen' is thereby proved" [Q.E.D.], p. 313.

*Canon II.—A statement that facts are only possible on Weismann's hypothesis is proved by repeating the statement with one or more of its terms replaced by synonyms or synonymous phrases.*

*Example.*—"Determinants [defined seven lines below as 'working living particles which operate during the course of development in a definite manner, so that the part they have to determine is thereby produced'] must be present because the parts of the organism are capable of individual and transmissible variation, and this is only possible if there are in the germ plasm living particles related to definite parts of the perfect organism" [i.e. *determinants*], p. 321.

*Canon III.—A fact contrary to Weismann's doctrine may be* (a) *dropped, or* (b) *reversed.*

*Example* (a).—"But further investigations by Blochmann and myself led to the discovery that parthenogenetic eggs only form one polar body, while those which require fertilisation always form two" (p. 324); [*the* FACT DROPPED *is that the bee's egg always forms two polar bodies, and yet will develope—into a drone—without "requiring fertilisation"*].

*Example* (b).—"Thus the conclusion has been gradually more and more firmly established that in the sex-cells of animals as well as in

[1] For the translation or mis-translation, however, Prof. Weismann, who was good enough to send us his German MS., is in no way responsible.—[ED. *Nat. Sci.*]

those of plants a reduction of the number of chromosomes takes place" (p. 325). [*The* FACT REVERSED *is that the reduction, be its mode and interpretation what they may, takes place in the* ASEXUAL *spore formation in Archegoniate plants, not in the formation of the* SEX-CELLS.]

*Canon IV.—The names of observers may be cited in support of a Weismannic interpretation of the facts, without mentioning that the observers themselves maintain that the facts are irreconcilable with these interpretations.*

*Example.*—"I need only mention the names of Boveri, Henking, Vom Rath, Rückert, Häcker, Korschelt, Ischikawa, and Van der Stricht; and in the domain of botany, Strasburger, Ischikawa, Calkins, Beliejeff, and Guignard." *This follows a statement* on the reduction processes in the nucleus: "I therefore altered my second hypothesis to a third, according to which both divisions of the ripening ovum together imply a reduction in the number of ids, and this position I still maintain" (p. 324). [*Yet the observations of Boveri, Guignard, and Strasburger are regarded by them as inconsistent with "this position."*]

*Canon V.—It is immaterial to acknowledge Weismann's debt to those who have pointed out the untenability of his former theories, and induced him to recast them on a sounder logical basis.*

*Example.*—"It is immaterial whether I myself or one of my sharp-sighted critics was the first to discover this mistake" (p. 326). [In what sense is it immaterial for the constructor of a theory to acknowledge his debt to those who point out that its foundations are unsound?]

QUEEN'S COLLEGE, CORK,
*May* 11, 1899.

# The Anal Glands of the Aphodiinae.

By Dr. L. Bordas.

The anal glands of the Aphodiinae differ essentially in their form and situation, and above all in their structure, from those of the other Coleoptera. While these organs generally consist either of a series of small spheres disposed in clusters, or of two long appendicular tubes (Dytiscidae, Gyrinidae), those of the Aphodiinae (*Aphodius fossor* ♂ L.), on the contrary, consist of a series of unicellular secretory acini—spherules provided with a very delicate canaliculus ending in an efferent cylindrical duct. This disposition reminds one of the form which obtains in the glands we have described in the Hymenoptera (Apinae and Bombinae), under the name of supracerebral glands (see *Ann. Sci. Nat.* [*Zool.*], 1894; "Appareil Glandulaire des Hyménoptères," Plate 1, Figs. 10, 11, and 15).

The anal glands are paired and open separately at two points very near each other on the basilar plate immediately under the chitinous case that constitutes the principal piece of the genital armature.

Each gland is composed of a multitude of unicellular acini, disposed in groups along the principal excretory duct of each gland. Those acini are generally spherical, and contain an internal granular protoplasm with a few peripheral vacuoles.

The nucleus that lies near the centre of each cell encloses several nucleoli. A very thin membrane surrounds each acinus, and is continued into an excretory canaliculus which is short and very thin. This canaliculus takes origin by several branches at the centre of the acinus. All the canaliculi open directly into two uniformly cylindrical tubes, which may be designated the excretory canals of the gland. There is no collecting reservoir as in the other cases. There is, therefore, as this short summary indicates, a quite special structure in this particular case. For further details I must refer to my memoir on the defensive glands of insects.

Marseille.

# FRESH FACTS.

Ventilation of Eggs. Oskar Schultze. "Ueber den Einfluss des Luftmangels auf die erste Entwicklung des Eies," *Verh. Phys. Med. Ges. Würzburg*, xxxii. 1899, pp. 191-202, 4 figs. When frogs' eggs are robbed of their normal supply of oxygen for a short time, the development is slowed. In more unpropitious conditions the development stops, but it may recover even after a stoppage of two days if good ventilation is restored. In most cases, however, the inhibition results in malformations, and eventually in death. The result may be due to poisoning with carbonic acid as well as to actual dearth of oxygen, for an organism will form $CO_2$ even in the absence of supplies of oxygen. It is safe to say that good ventilation is an essential condition of normal development from the time of fertilisation onwards.

Aplysiopurpurin. C. A. MacMunn. "The Pigments of *Aplysia punctata*," *Journ. Physiol.* xxiv. 1899, pp. 1-10, 2 pls. Aplysiopurpurin is an extremely unstable pigment secreted by the dermal glands of *Aplysia*, and varying slightly in different species. It can be isolated in a pure, or at least an approximately pure condition, by means of precipitation with ammonium sulphate and further treatment described in the paper. The integument of *Aplysia* contains pigments which appear to be of an excretory nature, besides a special violet pigment.

Genital Organs of Holothurians. L. Bordas. "Recherches sur les organes de la génération de quelques Holothuries," *Annales Faculté des Sciences Marseille*, ix. 1899, pp. 187-204, 1 pl. In this research, which the author has sent us from the laboratory of Endoume, the structure of the reproductive organs in *Holothuria tubulosa*, *H. poli*, *H. impatiens*, and *Stichopus regalis*—all abundant in the Gulf of Marseilles—is described. Macroscopically, this is very simple, and almost the same in both sexes. Microscopically, the tubes consist of germinal epithelium on a basilar membrane, then a connective fibrillar layer, then a layer of circular muscle-fibres (outside which there may be longitudinal fibres), and externally a layer of cylindrical ciliated cells. Dr. Bordas commends the edibility of the organs as not inferior to those of sea-urchins, which many find appetising.

Struggle for Existence. F. P. Bedford. "Gecko Cannibalism," *Nature*, lx. 1899, p. 8 (Letter). In a stomach of a young female gecko (*Gecko monarchus*) there was found a smaller gecko of the same species. As the writer says, "the fact that an animal will prey upon another of its own species, while living under completely natural conditions, and with an abundant supply of its normal insect food, seems worth recording."

Enterochlorophyll. C. A. MacMunn. "On the gastric gland of Mollusca and Decapod Crustacea: its structure and functions," *Proc. Roy. Soc.* lxiv. 1899, pp. 436-439. Dr. MacMunn has been forced by his observations to believe that enterochlorophyll is a pigment which primarily has been taken up

from the intestine dissolved in a fatty medium, and is carried, either by leucocytes or in some other way, to be deposited with the fat and perhaps other reserve products in the gastric gland. Whether it is utilised for the production of other pigments or not is a question for future investigation. That it is a chlorophyll derivative he now believes to be proved. Its stability, as compared with plant chlorophyll, is due to the fact that it has been altered by the action of the digestive juices.

SHELLS IN OCTOPODS. A. APPELLÖF. "Ueber das Vorkommen innerer Schalen bei den achtarmigen Cephalopoden (Octopoda)," *Bergens Museum Aarbog.*, 1899, pp. 1-15, 2 pls. It is usually said that Octopoda differ from Decapoda in the absence of an internal shell. There is, however, such a structure in a cavity on the dorsal surface of the mantle in the aberrant Cirroteuthidae, and Appellöf interprets in the same way two narrow chitinous rods in Octopodidae. The cavities in which these rods lie are lined by an epithelium which secretes the concentrically laminated chitin.

A TOOTH IN A TESTICLE. A. E. METTAM. "Dentigerous Cysts," *The Veterinarian*, lxxii. 1899, pp. 309-312. In this interesting note Prof. Mettam describes *inter alia* a case in which the testicle of a horse, removed during an ordinary castration, contained a fully developed canine tooth with a bony alveolus enclosing its fangs.

A RECORD HOMER. T. FOWLIE. "Vitality of Pigeons," *The Veterinarian*, lxxii. 1899, pp. 316-317. As surgeon for the Aburn Homing Club, N.S.W., the author has seen several instances of marvellous vitality and recovery from severe lacerated wounds in pigeons. One bird, "a record homer," had the muscles of the neck terribly lacerated, the trachea and oesophagus exposed from the pharynx to the clavicles, the ingluvies laid open and a third of its walls destroyed. The wounds were treated and recovery was complete. The bird has since reared one brood, and is going to race again. What astonishes the author most is that the healed ingluvies "can be of no practical use as an organ of digestion;" but who said it ever was?

GEOLOGY OF THE MALAYAN ARCHIPELAGO. B. KOTO. "On the geologic structure of the Malayan Archipelago," *Journ. College of Science Tokyo*, xi. 1899, pp. 83-120, 1 p. In this welcome contribution Prof. Koto gives a brief sketch of the geologic and tectonic structure of the island-world of South-Eastern Asia, usually comprehended under the name of the Malayan Archipelago. His primary motive was a desire to compare the physical features of the island of Taiwan with those of the Far Eastern Indies, and the inquiry has broadened out. As he says, the great Viennese geologist, Prof. E. Suess, has already drawn a lively picture of the structure of these islands in his "Antlitz der Erde," but this account has been enlarged and modified in the paper before us.

PTERYGOTUS AT MELBOURNE. F. MCCOY. "Note on a new Australian *Pterygotus*," *Geol. Mag.*, N.S., dec. iv. vi., May 1899, pp. 193-194, 1 fig. No merostomatous arthropod has previously been recorded from Australia. This specimen described as *Pterygotus australis* n.sp., is about half a somatic segment in black flaggy Silurian (s.str.) rock, from the main sewer tunnel in Domain Road, South Yarra. It was collected by F. Spry, who has presented it to the Melbourne Museum.

# SOME NEW BOOKS.

The Native Tribes of Central Australia. By Professor Baldwin Spencer and F. T. Gillen. Pp. xx. + 671, with 133 figures, 2 maps. London: Macmillan and Co., 1899. Price 21s.

If we may judge of the progress and vitality of a science by the nature of the literature called forth in response to the demand for information, we may assuredly regard the scientific study of man, hampered though it be by the cumbrous title of anthropology, as one of the most progressive of the sciences. Anthropologists have certainly responsibilities of a serious kind, and it is one of the most curious phenomena of research, that the scientific study of man himself, a study of such great and varied interest, and of such wide practical importance, should have to rank as almost the youngest of the sciences. Fortunately its youthful age is now associated with a youthful vigour which promises well for the future; but, at the same time, it must be admitted that every year, every month in fact, lost in delaying the active prosecution of scientific inquiry into the physical characteristics, customs, and arts of the lower races of mankind, and their relation to their environment, renders the task of the science more difficult, owing to the vanishing of the materials for study. The "civilisation" of the savage not only means the substitution of the customs and arts of the more cultured races for those of primitive peoples, but too often implies the extinction of the lower race itself as a race, it may be by a gradual process of absorption into a pseudo-civilised state, and the suppression of normal racial characteristics, it may be by actual extermination. We may well rejoice at the present signs of activity in the prosecution of anthropological research in Australia. The recent "Horn" expedition, carefully organised for scientific investigation, was brought to a successful issue, and produced excellent results, and the report on the anthropology of the districts visited contains valuable materials brought together by Messrs. Stirling and Gillen. For some while Brough Smyth's "Aborigines of Victoria" was the classical work upon Australian native life, but later this was followed by splendid work on the part of Messrs. Howitt and Fison, work of a most painstaking and sterling character. Quite recently Dr. Roth published his observations upon the natives of the north-eastern regions, an excellent and most useful monograph, bringing to light many new facts, and stimulating afresh the interest in Australian anthropology. Now we have in our hands the volume recently produced by Messrs. Spencer and Gillen. Of this work it is difficult to speak in terms of sufficient praise, a volume of upwards of 650 pages, positively packed with information of a valuable kind. One may fairly regard this book as one of the most important contributions to anthropological literature of recent years. Prof. Spencer has brought to bear upon the subject his training as a scientific biologist and skilled observer, and it is indeed fortunate that he has been able to devote so much of his time and energies to the study of man. Mr. Gillen has long been resident amongst the natives of Alice Springs, as a protector of aborigines, and has had,

and made use of, unrivalled opportunities for acquainting himself with their manners and customs, and the inner workings of the savage mind.

These two observers have collected an enormous mass of facts, a large proportion of which have come under their personal observation, and the result of their searching inquiry is that they have been able to carry us far behind the scenes of savage life, and to supply much long-needed information concerning the *arcana* of Australian savagedom.

The social organisation of the tribes is described in detail, starting with the fundamental division of the tribe into two exogamous intermarrying groups, which may be further divided, but whose former existence is always manifest. The elaborate systems of relationships between members of the tribe become more and more complicated as the divisions become more complex, and the native terms of relationship become very numerous. It is necessary to use the native terms in dealing with relationships, for many of them have no equivalent in English. The laws regulating marriage are very strict, and a very limited power of selection is allowed to the native in search of a mate. Totemism enters largely into the social system, and is one of the dominant factors in social regulations. The totemic system is one of great interest in this region, and the information collected thereon by the authors of this work is of very great importance in relation to the general subject of totemism. The laws regulating food restrictions in relation to the eating of the totem animal or plant are very significant, and point to a gradual development from a period of greater freedom in the matter. The statement that, in the Arunta tribe, the child belongs to the totem of the spot at which the mother believes that it was conceived, is one of great interest. It is clear that the totem is not inherited from either father or mother, and that the whole matter is bound up in the doctrine of reincarnation of ancestors, whose spirits have remained at certain spots in the sacred *churinga*. From the *churinga* the spirits of the Alcheringa ancestors and their descendants pass into women visiting the sacred spots, and hence the child belongs to the totem of the place itself, which is the totem of the bygone tribesmen whose spirits have continued to inhabit the *churinga* deposited there.

These *churinga*, equivalent to the so-called "bull-roarers," are described in a manner which shows how vastly important a part they play in all the mysterious ceremonial of the natives. They may never be seen by women. No such complete description of their use exists elsewhere, although Messrs. Howitt and Fison and others have shown their great importance, and the mystery surrounding their use.

The history of a tribe is referred back to the Alcheringa days, a time long past, in which the ancestors of the tribe are supposed to have existed as a mythical, semi-human race. The wanderings of the Alcheringa ancestors are carefully treasured up in the native memory. These traditions are of use as pointing to the probable migration routes of the various tribes and sections of tribes, and to the development of their culture and ceremonial observances.

The complicated, severe, and prolonged initiation ceremonies are recorded in the book with great care and precision, all the phases being described with a minuteness of detail which reflects great credit upon the observers.

It would be impossible, in this notice of the book, even to touch briefly upon the numerous points of interest raised in this work, and no attempt can be made to do so. The whole forms a very concise account, and is, moreover, liberally furnished with illustrations prepared from photographs. They are for the most part very good.

If a work of this kind still leaves us with the feeling that there is yet a vast amount to be learnt from the natives, as regards their customs and beliefs, and the esoteric side of their culture, this is no fault of the authors. We will hope that the example set by them may be followed by others, while there is yet time to study primitive native life in its natural environment.

HENRY BALFOUR.

## PROGRESSIVE VULCANOLOGY.

Volcanoes. By Prof. BONNEY, D.Sc., F.R.S. 8vo, pp. xvi. + 351, with 12 plates, 21 figures, and a map. (The Progressive Science Series.) London: John Murray, 1898. Price 6s.

Volcanoes are of interest to so wide a circle of general readers, and, at the same time, they present so many problems which engage the attention of men of science, that one is often led to wonder that, numerous as are the books already published about volcanoes, there are not more of them still. Historian, poet, mythologist, artist, geographer, and geologist, all find in this one subject matter of perennial interest, each according to his own special line of thought. Little wonder, therefore, that the authors of the various books that have been written about volcanic phenomena have treated their subject in styles so diverse. Little wonder, also, that when a new book on the subject appears, the majority of its readers, while grateful to the author for presenting them with well-known facts in a new light, and for telling them much about other matters that they had not known of before, yet feel that they are not quite satisfied. Whether it is possible for any man to write a book on volcanoes which shall give universal satisfaction is another matter—perhaps it would be safe to say that it could not be done, because no two thinkers ever regard the subject in exactly the same light.

Whoever undertakes to write a book on volcanoes, should, there can be no doubt, regard them quite as much in relation to both the future and the past, as in their relation to the present; and, moreover, should be capable of regarding them quite as much from the standpoint of the astronomer as from that of a geologist. It is not easy to find such a man, and it may be long before any such Newton arises amongst the students of vulcanology. But Professor Bonney's wide experience as a lecturer and writer on geological subjects in general, and especially on the petrographical side of geology, gives him a claim to write with authority on the present subject which is stronger than most other men possess; and, assuredly, most people will be agreed that it is only to a geologist that such a task should be committed.

In planning the work, Professor Bonney begins by giving an account of a "living volcano," taking, as most of us would naturally do, Vesuvius as his type. Now that photography has made us familiar with almost every detail regarding this grand Neapolitan volcano, there is a peculiar fitness in the author's choice of this as his first example. He then takes us to the Phlegraean Fields, and gives us an account of the dormant and extinct volcanoes of that most interesting region. Then Stromboli is noticed as an example of an active volcano of a different type, while a brief account of the terrific eruption of Krakatau or Krakatoa serves as a description of an explosive outburst. Then follows a description of the quiet effusive eruptions of the Sandwich Island volcanoes, which are of so much importance to geologists, as illustrating the formation of extensive fields of gently-inclined lavas, such as characterise the plateau type of volcanic deposits.

One would naturally expect Professor Bonney to give an excellent account of the materials of volcanic rocks, and accordingly Chapter II. is devoted to this important part of the subject. It is very pleasant to find him (p. 66) entering a much-needed protest against the unnecessary introduction of new names for rocks, a practice which bids fair to land us before long into a state of chaos little better than that to which the nomenclature of ammonites has been degraded.

Next follows an account of extinct volcanoes, in connection with which are given some reproductions of Dr. Tempest Anderson's beautiful photographs of volcanic phenomena. Readers of Prof. Bonney's book will cordially agree with

the author in hoping that Dr. Anderson will shortly see his way to publishing a large number of these instructive and beautiful views.

Frequent references are made to Sir Archibald Geikie's "The Ancient Volcanoes of Great Britain" in this chapter. But surely Sir Archibald is not responsible for the statement made on p. 147, that most of the lavas on Arthur's Seat are "porphyrites." There is an excellent account in Chapter II. of the exposure of volcanic rocks on the Fifeshire coast, which are probably amongst the best examples of their kind in existence.

On pp. 162-164 the author makes some reference to the physical relations between the bounding surfaces of intrusive masses and those of the rocks they invade. In connection with this it is well known by many field geologists that this relation is one which indicates the replacement of the one by an equivalent volume of the other. Explain it how one may, there can be no doubt about the actual fact. Many readers will think that the author might have made some fuller reference to this especially interesting point, even if, in doing so, less had been said about the so-called "laccolites."

Chapter IV. gives an excellent summary of the chief points of interest connected with the oldest British volcanoes.

Then follows an account of existing volcanoes, considered mainly in reference to their geographical relations.

The concluding chapter, which is perhaps that which will prove of most interest to all, deals mainly with theoretical considerations. It may fairly be described as an admirable summary of the chief points of interest in this connection. As the reader puts the book down he cannot help feeling that if, here and there, his own way of viewing the facts may be somewhat different from the author's, yet the book as a whole is full of suggestive matter, and will form an admirable companion volume to the works of the best previous writers on vulcanology. J. G. G.

## HISTORY OF NATURAL SCIENCE.

Grundriss einer Geschichte der Naturwissenschaften, zugleich eine Einführung in das Studium der grundlegenden naturwissenschaftlichen Litteratur. By Dr. FRIEDRICH DANNEMANN. 8vo. Leipzig: Engelmann.

Band I. Erläuterte Abschnitte aus den Werken hervorragender Naturforscher. Pp. xii. + 375, with 44 figs. 1896. Price 6 Marks.

Band II. Die Entwickelung der Naturwissenschaften. Pp. 435, with 76 figs. and a spectrum plate. 1898. Price 9 Marks.

In the young student's haste to gorge his brain with facts which are presented to him in abundance, and even in his maturer haste to hunt and discover for himself, little opportunity is left for an adequate appreciation of the history of science. Every one admits the grandeur of the historical development, and its importance is recognised by all who use the evolution idea as a formula in thinking, yet it seems impossible to avoid the confession that the historical aspect of science receives comparatively little attention in this country. In many a text-book there is, except in the bibliography, hardly a suggestion that the subject has had a history at all; in many a course of lectures it is the same; and the student receives his up-to-date science like a translucent and differentially-stained histological section dropped down on his slide by a demonstrator with a bowlful—a section so beautiful and clear, that in his saner moments the ingenuous beginner does sometimes wonder how it came to be as it is.

With the laudable aim of lessening this defect in our scientific education Dr. Dannemann has prepared the two volumes before us, which he modestly recommends to all interested in the methods and results of exact science. In the first volume, he pursues a very interesting and natural method, that of

giving a series of representative extracts from the writings of prominent workers, from Aristotle to Humboldt, sixty-two in all. To criticise these seems hardly fair without making a better selection of sixty-two, and this we have no desire to attempt. Just as in the case of a "Golden Treasury of Lyrics," one misses some old favourites, and wonders what possessed the editor to put others in, one misses anything from Harvey, Buffon, Lamarck, or von Baer, and how many others!—but room had to be found for Euler and Galvani, Wöhler and Carnot. Our disappointment is softened too when we come across Swammerdam and Trembley, Goethe, and Sprengel, Schwann and Schleiden. In short, though I daresay a man might have made a better selection of sixty-two samples, the fact is that no one ever did. Dr. Dannemann speaks in a quiet way in his preface of the book being useful to pupils in the upper forms of high schools, but shall we not be frank and admit that there is none of us—Herren Professoren, Doctoren, Privat-Docenten, and Gelehrten in general—who would not be the better of reading a book like this, and renewing our youth thereby.

The second volume is more ambitious and more open to criticism, to which we are averse. Its title is "The Development of the Natural Sciences," and we doubt whether the theme is within any one man's grasp, though Mr. Merz made a brave beginning in the first part of his "History of Scientific Thought in the Nineteenth Century" (1897). Dr. Dannemann divides his history into four chapters:—Ancient Times, The Middle Ages, The Later Times, and Modern Times.

In regard to the last—the scientific Plistocene—he notes especially the results of the theory of combustion and atomicity; the discovery of electricity and its consequences; the influence of Laplace and Herschel on astronomy; the further progress of chemico-physical research; the institution of the "natural system" in zoology and botany; the results of the establishment of the doctrine of the conservation of energy; the influence of chemico-physical research on biology and geology; and the evolution-doctrine. But this is far too much for about 100 pages; and although the author faces up manfully, we are afraid that it won't do. There should at least have been a separate volume for the Darwinian era, for with Darwin all things became new. We liked the book very much until we reached what we have called the Plistocene period, and then, if the erudite author will forgive us, we liked it very little. The task was too big for the pages. May we hope for a second edition in three volumes? The publisher, who has already the gratitude of the historically-minded by his issue of Ostwald's "Klassiker," should see to this.

J. A. T.

## ENGLISH EDITION OF VERWORN'S GENERAL PHYSIOLOGY.

General Physiology: An Outline of the Science of Life. By Max Verworn, M.D., Ph.D., A. O. Professor of Physiology in the University of Jena. Translated from the Second German Edition, and edited by Frederic S. Lee, Ph.D., Adjunct Professor of Physiology in Columbia University. 8vo, pp. xvi. + 615, with 285 illustrations. London: Macmillan and Co., 1899. Price 15s.

As the first and second German editions of this important work have been already reviewed in *Natural Science*, we must restrict ourselves to welcoming the book in its English dress. And this welcome should be a hearty one, for the volume occupies an almost unique place as an introduction to the problems of general physiology, and has been widely appreciated for its lucidity and suggestiveness. As Mr. Lee says in his prefatory note, "Many of the special views of the author have encountered opposition—a fact that perhaps is indicative of their value—yet, however much we may agree or disagree with him

upon special points, we all must acknowledge his breadth and be grateful to him for presenting such a wealth of facts, and for pointing out so clearly the possibilities of research." Mr. Lee's translation is admirably done, and sustains the spirit and charm of the original. Opinions must of course differ in regard to particular phrases, *e.g.* "the mechanics of hereditary transmission" for "Vererbungsmechanik"; or "structure and liquid" for "Structur und Flüssigkeit"; but these are trivial matters when the whole book is so clear. A new feature of merit is the bibliography at the end, to which the references in the text are related by placing the year of publication after the author's name, a convenient method which makes its way slowly in Britain. There is still what seems an unnecessary repetition of the same figures in different parts of the book, and the German edition is followed in printing the names of classes and the like in the same type as names of species, which also seems to us a mistake. But these are paltry criticisms. What we have to do is to recommend with all possible heartiness this new edition of a book which many already know to be fascinating. X.

## OPTICAL INSTRUMENTS.

Die Optischen Instrumente der Firma R. Fuess, deren Beschreibung, Justierung und Anwendung. By C. Leiss. Pp. xvi. + 397, 233 figures and 3 plates. Leipzig: Wilhelm Engelmann, 1899.

This book is one of a type unknown in British scientific literature. As its title indicates, it contains a full description of the optical instruments supplied by the well-known firm of Fuess. The question at once arises, How far can such a book be of use in an optical laboratory? The natural impression might be that its usefulness must be extremely limited, that it can be of little use to those who are furnished with the instruments of other firms, or even with those of Fuess which are not of the most recent construction. Yet this impression would be extremely false. All scientific instruments, which are designed for definite purposes, are designed upon definite principles. And in this book the principles are fully treated. The result is that variations in the details of construction are of little importance. The description is aided by no fewer than 233 elaborate figures.

There are many students and others, who, having become acquainted with the elements of physical science, desire a more complete acquaintance with the methods of physical research in order that they may follow more intelligently the progress of scientific knowledge, and who are unable to make themselves practically acquainted with these methods by work in a laboratory. To these, a book of this kind should be of great use.

## CONDITIONS OF GROWTH.

Experimental Morphology, Part II.: Effect of Chemical and Physical Agents upon Growth. By C. B. Davenport. Vol. II. Pp. 281 to 509. London: Macmillan and Co., 1899. Price 9s.

This volume is the sequel to a previous one published two years ago, dealing with the effects of chemical and physical agents upon protoplasm; and in all respects the new-comer is the worthy partner of the first.

Mr. Davenport's work is of high importance. There is no other of the same type, dealing with the same matter from the same point of view. In the second place, the point of view is that of the "later biology" such as has taken form in the work of Roux and of the German school of which he is the very energetic and hard-working chief, and in the work of a number of zoologists, in the English-speaking countries principally, Mr. Davenport himself being *unus ex pluris* of the latter, and *pars non parva*, by his personal investigations.

"Experimental Morphology" is a very suggestive book—a book which gives

ideas, which opens new vistas, which suggests new experiments. To the neophyte it opens quite a new train of thought; it shows what ought to be done, what has been done, and what could and should be done. For, most certainly, Mr. Davenport's book is of the provisional and temporary sort: before ten years he will have to begin it again, adding to each chapter, and opening new chapters, if only we, his readers, do read him as we should, and take the hints which he gives at each page. This "if," however, is a mere "oratory precaution"; there is no doubt as to the future of experimental morphology; the number of workers in the field is steadily increasing.

The matters considered by Mr. Davenport in this second part of his book are all related to growth. Growth is an important subject-matter, practically as well as from the speculative standpoint, and it is a very important matter that the agencies which exert an influence on growth should be clearly recognised. The agriculturist and breeder are as much interested in this as the physician and biologist.

The agencies referred to are numerous, for "that property which protoplasm among all substances alone displays of increasing itself for an indefinite time and to an indefinite amount" is much influenced by external conditions: by chemical agents, by water, by the density of medium, by molar effects, by gravity, by electricity, by light, in brief, by complex agents; and each of these conditions is the subject-matter of a chapter in Mr. Davenport's book.

Generally speaking, of course, each chapter is a summary, an abstract of the work which has been done by various workers, the author himself often included, and a bibliographical list of references is the necessary complement of the text. It must be said that no important paper seems to have escaped Mr. Davenport's notice. However, in some cases, there are references which ought to be taken account of. In the chapter where the author speaks of the chemicals and of their distribution among organisms, no reference is made to Fliche and Grandeau's investigations concerning the chemical composition of various species of plants nearly related to each other; nor to recent investigations concerning the presence of copper in plants. Of course, some of the latter are too recent to have been included in the book, as they have been published only two months ago (in *Botanical Gazette*, by D. G. MacDougal); but others are of older date, such as those of J. B. Skertchly and Lehman; others are to be found in Schimper's recent *Pflanzen-Geographie*.

Concerning the effect of flowing water upon the direction of growth, I would also refer Mr. Davenport to a paper of Arnould Locard on the fauna of the water-pipes in Paris; the French malacologist observes that in the water-pipes, where the flow is rapid, *Dreissensia polymorpha*, which is abundant, relinquishes its curved form to become straight, and other mollusks—*Lymnaea*, for instance—assume a specially elongated form under the same influence.

And concerning the effect of light, and especially monochromatic light, some interesting facts are to be found in Finsen's investigations. The latter have been recently summarised and brought together in a pamphlet on Phototherapy.

It must not be inferred from the preceding lines that Mr. Davenport is not well informed; and while I refer him to the facts and papers mentioned, I only wish to give what help and aid a reader should always give an author when he meets with a carefully constructed work in which he takes a great interest. This is co-operation, not criticism.

A review of Mr. Davenport's book is hardly possible, in what space is available; it is so very analytical that, of course, every biologist should read it for himself, and make use of it for new experiments. There are any number of points which require new and full investigation. For instance, the influence of light. Retardation of growth is induced by light in most aerial plants, while in animals acceleration obtains; and in both cases the effect is due to the same rays, the chemically active. This is the general fact: a full study of the matter would certainly prove very profitable.

Very useful also would be an exact investigation of the period of life of any organism during which the influence of any agent would prove most effective—whether good or bad—on one system, or function, or organ.

But no one can read this book without perceiving, by what has been done, what remains to be done; it is an eminently suggestive work, and therefore it is as well to leave the reader to draw his own conclusions. We earnestly hope, for the sake of biology, that the book will be extensively read by the younger workers: most certainly it will open their eyes to new views, and push them in a field where much is to be done, and a good harvest reaped. Mr. Davenport deserves the thanks of all who are interested in biology.

H. DE VARIGNY.

## "KORSCHELT AND HEIDER."

Text-Book of Embryology of Invertebrates. By Drs. KORSCHELT and HEIDER. Vol. II. (Phoronidea, Polyzoa, Brachiopoda, Crustacea, Palaeostraca.) Translated by M. BERNARD. Revised and Edited by MARTIN F. WOODWARD. Pp. xvi. + 370 and 165 figs. London: Swan Sonnenschein and Co., 1899. Price 12s.

If writing a text-book be a thankless task, the translating of a foreign text-book must be worse. In such a subject as embryology, the functional life of a text-book may be put down as about two years, so that a translation really requires to be practically re-written. An attempt has been made by Mr. Woodward to meet this want by the addition of footnotes, but we think it would have been better to have taken the bull by the horns and have re-written the whole. In some cases we find that a note by the revising editor at the end of a page or two of "general considerations" leads one to condemn the latter as "premature conclusions." This is specially evident in the first and the last groups dealt with, namely, the Phoronidea and Palaeostraca. From these remarks it will be seen that Mr. Woodward's notes add greatly to the value of the book in bringing it up to date and in reducing the relative antiquity of the work to a minimum.

A perusal of the discussions upon phyletic affinities leads a thinking zoologist to pause and consider whether zoological science is yet sufficiently advanced for the determination of these relationships. Do we not require to definitely settle what features are to be regarded as of phyletic value? We find the authors arguing for the unity of the Molluscoida as a natural group, firstly, on account of structural resemblances (pp. 84 to 88) which, if one allows for similarity of habitat, resolve themselves into a well-developed coelom with similarly related divisions. The absence of a true coelom again assists the authors in isolating the Entoprocta though they commence their discussion of the Phoronidea by the remark:—"The Actinotrocha may without difficulty be regarded as a somewhat modified Trochophore." This becomes modified later into (p. 86) "The Actinotrocha type is distantly allied to the Trochophore larva, being distinguished from the latter chiefly by the presence of a true coelom," and this "distant alliance" from Brachiopoda through Actinotrocha to Trochophore is used as an argument against the comparison of the former with *Sagitta*. The structural resemblances between Brachiopoda and Chaetognatha are dismissed as "analogies," and their differences "in manner of life" are quoted against genetic affinity! Surely structural resemblances combined with differences in habitat would form the strongest arguments for genetic affinity.

The immense number of facts and observations clearly set forth will place this "Embryology" in a unique position, and the work of translation has been ably and fluently effected. All the diphthongs are abolished, and such words as "zoaea," "coelom," "Palaeostraca," "zooecium," and "caenogenetic" have a

peculiar appearance to some eyes, though not to readers of *Natural Science*. We may also note "oral and anal apertures" for mouth and anus, "Phoronidae" for Phoronidea, "epistomal" for epistomial, "endosternum" for endosternite, and "intestineless" for anything more euphonious. Such terms as "unicorneal" and "primary coelom" also require amending.

The illustrations are numerous and good, and in instances of the contrary, such as Fig. 8, Mr. Woodward's notes save misunderstanding. In this connection, however, the caricature of *Cephalodiscus* on p. 89 should have been excised.

In making these critical suggestions we do not wish to lose sight of the outstanding merit of the work and the great benefits which the translator and editor have bestowed upon British zoologists by their labours, which, considering the nature of the task before them, could hardly have been more satisfactorily accomplished. We heartily congratulate them on the production of this volume, which must tend to make "Korschelt and Heider" more readily accessible to a number of embryological workers who have not the time, and in many cases the training, to follow the authors in the original.

A. T. Masterman.

## THE GEOLOGY OF SOUTH AFRICA.

Bibliography of South-African Geology. Compiled by H. P. Saunders, Secretary to the Geological Commission. 8vo, 56 pp. South-African Museum, Cape Town, 1897.

Cape of Good Hope Geological Commission, Department of Agriculture. First Annual Report (for 1896). Large 8vo, 52 pp., and 2 plates, 1897. Second Annual Report (for 1897), 86 pp., and 2 plates. Richards and Son, Cape Town, 1898.

We gladly welcome these first-fruits of the Geological Survey of the Cape of Good Hope Colony, established in 1895. The official work of Dr. G. S. Corstorphine, and his assistants, A. W. Rogers and E. H. L. Schwarz, has been especially directed to the elucidation of the structure and physical features of the south-western part of Cape Colony. This includes the Divisions of Robertson and Worcester, and parts of Stellenbosch, Prince-Albert, Ladismith, and Swellendam; or, in other words, "from the west coast eastwards to the Gamka River, and from the Zwartebergen, on the north, to some distance south of the Langebergen; while the Malmesbury district has been completed north to St. Helena Bay." This area exhibits the following formations in *ascending* order:—

1. Malmesbury Beds.—Non-fossiliferous slates, phyllites, mica-schists, and quartzites, with intrusive granite, quartz-porphyry, and diabase.
   (Marked unconformity with the next exists.)
2. Table-Mountain Sandstones.—Sandstones and quartzites, with occasional shales.
3. Bokkeveld Beds.—Fossiliferous shales, sandstones, and greywackés.
4. Witteberg Beds.—Mainly quartzites (often with alga-like markings).
5. Dwyka Series.—Conglomerate and interbedded shales.
6. Ecca Beds.—Shales and sandstones, with occasional plant-remains.
7. Superficial deposits.—Gravels, sands, conglomerates, sandstones, and limestones.

The Surveyors have arrived at some new and valuable conclusions, determining the exact relationships of the Malmesbury and Ecca Beds in the Worcester Division, and of the Table-Mountain Sandstone in the Langeberg and Zwarteberg ranges. The great amount of folding to which the strata have been subjected, particularly in the mountains above mentioned, and in the Hex-river mountain, and the extensive dislocation, such as the "Worcester-Robertson"

fault, which has "the enormous downthrow of at least 9000 feet to the south," have been carefully worked out, and are clearly shown in the geological map and sections.

The Surveyors have conscientiously compared the notes and descriptions by earlier observers with the sections and other features which they themselves have studied and illustrated with precision. Being well-trained geologists they have drawn valid conclusions; for the most part confirming and enlarging those arrived at by A. G. Bain and other early observers. Mr. Saunders's *Bibliography* is a great help in referring to what has been published since 1841 (Clarke), 1844 (Darwin), 1845 (Bain), etc.

Summaries of local work by Messrs. Rogers and Schwarz are given in the First Report. A good map, with sections, illustrates the distribution of the dolerites in the Nieuweld Mountains, as described by Mr. Schwarz in his account of the district of Beaufort-West. There is also an elaborate sketch-plan, by Dr. Corstorphine, of the Cango Cave in the dolomite, near Oudtshoorn, south of the Zwartebergen.

In the Second Report a very welcome geological map of the district already surveyed (as defined above) is given, on a scale of 1:800000 (1 inch to 12·62 English statute miles), in Plate I. Five long and very instructive geological sections, from north to south, on a scale of 1 inch to 1·878 miles, or 800 Cape roods, are given in Plate II. In this Report Mr. Rogers gives an account of the Survey of the Stellenbosch district, and Mr. Schwarz that of the Robertson and Swellendam Divisions; and both together draw up a summary of the work done during 1897 in the area between the Langebergen and the Karoo. Here occur the Table-Mountain Sandstone, the Bokkeveldt and Witterberg Beds, the Dwyka Series, and the Ecca Beds; more especially illustrated in Sections 3, 4, and 5 of Plate II. The Dwyka Series of shales and conglomerates, lying conformably on the Witterberg quartzite, in particular is described, Mr. E. J. Dunn's careful notes being largely incorporated. The glacialised condition of the materials of the conglomerate is accepted; these are pebbles and fragments of "several varieties of granite, . . . several kinds of more basic igneous rock, sandstones, quartzites, argillaceous and calcareous rocks, and vein-quartz. Under the microscope the matrix is seen to consist of "minute fragments of anisotropic substances, . . . with quartz, felspar, . . . garnet, epidote, augite, . . . white and brown mica, sphene, magnitite, . . . calcite, as pebbles and grains, and as a secondary product." Included in the Dwyka Series are some peculiar superincumbent shales and sandstones, succeeded by black graphitic and pyritous shales; above these are the conformable Ecca sandstones and shales, containing plant-remains, and lying in folds parallel with the mountains, Zwartebergen, etc., on the south. To the north the great Karoo Series is present, and, together with the Eastern Province, will be reached by the Survey in due course.

At pages 37-43 Dr. Corstorphine supplies an interesting and philosophical review of the geological history of the successive formations constituting the country which the Geological Survey as yet has been able to determine and describe. T. R. JONES.

## SCIENCE FOR THE YOUNG.

Early Chapters in Science. By Mrs. W. AWDREY. Edited by W. F. BARRETT. 8vo, pp. xviii. + 348, with figs. London: John Murray, 1899. Price 6s.

The object of this book, writes Prof. Barrett, is to provide young people, especially the junior classes in schools, with an introduction to the two great divisions of science—Biological and Experimental—to the World of Life and the World of Experience—and, in a word, to cover the ground of Mr. Paul Bert's "First Year of Scientific Knowledge" in a more attractive

style and a less indigestible manner. The various sections have been revised by specialists—all Dublin men—whose labours are gratefully acknowledged; and the claim of the editor that the young reader will not have much to unlearn is fully justified. The style is fluent, yet not diffuse; the facts are well selected and well expounded. The zoology is particularly interesting, and contains a new parrot story of distinct merit. The figures are poor, shaded, yet without the least perception of tone-relation, like those of a century ago. Yet they are not without some spirit: the leary lemur, the supercilious tapir, and the 'orty proud gazelle, will be attractive to children; but the tusks of the hippopotamus are drawn *outside* the upper lip; the lion (*passant*) has a true heraldic air; and the "baboon," which will suggest a toy-shop poodle to the child, recalls to ourselves the illustrations to the old editions of Marco Polo or Sir John Maundeville. Again, of the three very bad figures of vegetable histology, that called "woody tissue" is easily worst, and a gratuitously useless disfigurement to the beautifully printed text. The paper is unduly thick, and the physical weight of the book (1 lb. 5 oz.) excessive; while the sheets are so carelessly stitched that it opens badly and will soon fall to pieces, just because of the merits of the letterpress. M.

## THE SECRET OF LIFE.

Die Kontinuität der Atomverkettung, ein Strukturprinzip der lebendigen Substanz. By Dr. Georg Hörmann. 8vo, pp. 118, with 32 figures. Jena: Gustav Fischer, 1899. Price 3 marks.

The aim of this pamphlet is defined by the author to be the application of Pflüger's hypothesis of the chemical continuity of protoplasm to various biological problems, with the view of showing that it renders these more readily comprehensible than any other hypothesis. Pflüger emphasised the power of polymerisation displayed by many organic substances, and suggested that in the living organism this might go on indefinitely so that great masses might arise, and the whole nervous system, for example, might be built up of a single giant molecule. Accepting this suggestion in its entirety, the author seeks in a series of chapters to show that it renders explicable such phenomena as the propagation of stimuli along a nerve, the effect of nerve section, the action of electrical organs, and so on. The pamphlet is illustrated by numerous ingenious and complicated diagrams, and may be recommended to those persons to whom such phrases as the "principle of chemical continuity" appeal strongly; to others we fear much of it will seem vanity and vexation of spirit. As in so many similar cases it is difficult to find out whether the author regards the hypothesis as merely of value in the construction of mental pictures of the more complicated phenomena of life, or whether he seeks to explain these phenomena in terms of chemistry and physics. We have noted what seems an excessive number of misprints, and cannot congratulate the author upon his style, which is not calculated to assist in the comprehension of a difficult subject. N.

## HISTORY OF ASTRONOMY.

A Short History of Astronomy. By Arthur Berry, M.A. 8vo, pp. xxxii. + 440, with 105 illustrations and nine portraits. (University Extension Manuals, edited by Professor Knight.) London: John Murray, 1898. Price 6s.

Unlike several books which have appeared within the last few years, in which the principal events of the history of astronomy have been described in the form of biographical sketches, the present work seeks to present the reader with a complete account of the historical development of astronomy without giving undue prominence to the biographical element. It is evident that Mr. Berry has most carefully prepared himself for his task not only by reading the

larger general works on the subject, but also by studying the chief monographs which have appeared on special periods, and also to some extent the original works of the great men who have guided science into new paths. The book does not suppose the reader to possess any (or at least but a very slight) previous knowledge of astronomy, and accordingly the first chapter, on "Primitive Astronomy," describes the celestial sphere and its principal phenomena. In the next chapter, on "Greek Astronomy," the pre-Alexandrian period might perhaps have been more fully dealt with, and here it appears that the author is not so well acquainted with monographs and special studies (*e.g.* those of Ideler, Schiaparelli, and others) as he generally seems to be. That he confounds Herakleides with Herakleitus is perhaps due to his having depended too much on Wolf. The account of the Alexandrian astronomy is clear and correct, and the same may be said of the following periods. Throughout the author gives a very correct picture of the state of science at the time under consideration, while his book is not only accurate but at the same time extremely readable. The astronomy of the last two hundred years occupies exactly half the volume, and both "gravitational," "observational," and "descriptive" astronomy are passed in review. The two latter departments are perhaps not quite so thoroughly discussed as the first one, and especially it seems strange that next to nothing is said about instruments. The numerous illustrations (including nine excellent portraits) are well chosen, and in many cases borrowed directly from the original works of Copernicus, Galileo, etc. Not only as a University Extension Manual, but also as a book suitable both for the general reader and for the more advanced student, the work deserves every commendation.

J. L. E. D.

## A TEXT-BOOK OF GENERAL PHYSICS.

A Text-Book of General Physics for the Use of Colleges and Scientific Schools. By C. S. HASTINGS, Ph.D., and F. E. BEACH, Ph.D. 8vo, pp. viii. + 768, with 495 figs. Boston: Ginn & Co., 1899. Price, 2 dollars 95 cents.

The reputation of the two Yale Professors led us to expect a thoroughly sound and interestingly written text-book on physics; and our hopes have been to a large extent realised. The book looks well, is finely illustrated, and contains some interesting novelties of treatment. How far the introduction of such novelties into an avowedly elementary book is expedient is a matter on which there will probably be great difference of opinion. Again, we very much question the wisdom of giving, *necessarily* without proof, Clausius' expression for the virial of a gas, evidently as a stepping-stone to an indication of how Van der Waals arrived at his famous equation, which after all is only of limited applicability. The chapter devoted to oboes, clarinets, French horns, saxophones, etc., might with advantage have been omitted, and the space utilised to far better purpose. The arrangement of the book is a little peculiar, and leads to the awkwardness of discussing electro-magnetic waves before waves in general have been taken up. In short, the interpolation of electricity and magnetism between elasticity and sound has nothing to commend it. There is also an unfortunate inversion in discussing magnetic induction and hysteresis before any account is given of electro-magnetism. These faults in arrangement may perhaps be covered by the statement in the preface that "the book as a whole is designed as an aid to the teacher," and "assumes as a necessary complement a course of [experimental] demonstrations." In other respects there is much in the book that is worthy of praise, one interesting feature being the use the authors make of the wave-front in discussing optical theories and applications. Students who have already gained some knowledge of physical facts and theories will find their grip of the subject strengthened by a careful perusal of this text-book, while the teacher will probably receive hints of no small value.

C. G. KNOTT.

## PHYLOGENY OF PLANTS.

Lectures on the Evolution of Plants. By Douglas H. Campbell, Ph.D. 8vo, pp. viii. + 319, 60 figs. London and New York: Macmillan and Company, 1899. Price 4s. 6d. net.

It is difficult to refrain from effusiveness in welcoming this book. Prof. Campbell has accomplished the feat of writing a botanical book which may be read right through by the merest zoologist or cultivated reader, and this with profit. His account of the vestiges of the evolution of plants—rather than of the evolution of plants—is admirable from all points of view. It would be absurd, of course, to claim that his opinions, or anybody's opinions, would receive a general assent, but the reader who wishes to get a general idea of what botanists think at the present day of the line of descent and succession of forms could not do better than read these lectures. He will perhaps unconsciously absorb a deal of good botany at the same time. As for Prof. Campbell's views one may summarise them in a few words. He thinks it "not unlikely that the separation of the two great branches of organisms, plants and animals, took place among the Flagellata." He regards the ciliated Volvocineae as primitive forms from the very frequent reversion to this condition exhibited by higher plants in their reproductive cells. The Chlorophyceae may be assumed to have given rise to the Bryophytes—the Phaeophyceae and possibly even the red Algae constituting an independent developmental line. The Archegoniate plants in general find their ancestors in the Hepaticae. Of the seed plants the Gymnosperms are obviously the lowest types, though it is not likely that they constitute a homogeneous class. "It is much more probable that they represent the remnants of two, and possibly more, quite distinct developmental lines. The cycads show close affinity with the true ferns, while the conifers recall more strongly the Lycopods."

There are few books of general botanical interest better worth reading than Prof. Campbell's excellent "Lectures." G. M.

## MILK.

Milk: Its Nature and Composition. A Handbook on the Chemistry and Bacteriology of Milk, Butter, and Cheese. By C. M. Aikman, M.A., B.Sc. 2nd Edition. Crown 8vo, pp. xx. + 180, illustrated. London: A. and C. Black, 1899. Price 3s. 6d.

The author's preface tells us that the aim of the volume is to give a short popular statement of the more important facts of the chemistry and bacteriology of milk. The book is essentially one made from books, and contains much useful information of a sketchy kind about milk, its various products and composition. Unfortunately, one finds that much of the information inserted in the chapters on bacteriology, which occupy 50 pages of the 174, has not been weighed by a competent authority. For example, it is indicated that tuberculosis is largely spread to human beings by milk. One also finds ridiculous statements such as, that Prof. M'Fadyean says "that one calf in every four is born tuberculous," that diphtheria and scarlet fever are transmitted directly from the cow to man, that the microbe of diphtheria has not been isolated, and that of all media milk is the best for growing microbes. These and other serious errors might easily have been avoided by exercising a little more care in compilation. S.

The Story of the Mind. By JAMES MARK BALDWIN. London: George Newnes, 1898.

Prof. Baldwin has given in this little book a pleasantly written series of chapters on psychology in its introspective, comparative, physiological, experimental, educational, and social aspects. The work closes with an essay on the genius and his environment. There is a well-marked personal note throughout, since many of the illustrations are taken from the author's own investigations; but this is quite in keeping with the form in which the work is cast. The only criticism that we feel called upon to make, is, that the book does not fulfil the promise of its title as we understand it. It does not present us with a continuous story of the mind, but with a somewhat discontinuous series of short essays on psychological methods and results. But it has good points.

*Bryotheca Bohemica* is a set of exsiccati, now being issued, at a price of 14 marks per century, by Dr. Ernst Bauer (of Smichow bei Prag, N.C. 961) in co-operation with Dr. V. Schiffner and others.

Professor Simon Schwendener, of Berlin University, has collected thirty-one of his botanical papers in two octavo volumes issued by Borntraeger, Berlin, at a price of 25 marks. The papers are reprinted without other alteration than that of printers' errors and mistakes in the calculations, and with the addition of a few notes. Reference is given in each case to the original place of publication, and reference will have to be made by the student in each case since the original pagination is not given. Except for this, all botanists will be glad to have these valuable memoirs in convenient form.

From June 1898 to March 1899, Mr. F. A. Bather communicated to the *Geological Magazine* a series of six illustrated articles on Wachsmuth and Springer's Monograph of "The North American Crinoidea Camerata." Since these contained more original matter than one looks for in a review, they have been furnished with an index and published in pamphlet form by Messrs. Dulau and Co., at the price of two shillings. The reprint also contains a portrait, obituary notice, and bibliography of Charles Wachsmuth.

The first fourteen pages of the *Proceedings of the Washington Academy of Sciences* were issued on April 14. They consist of the "First annual report of the Secretary," by G. K. Gilbert. This gives a history of the gradual multiplication and specialisation of scientific societies in Washington, and their eventual federation under an Academy in February 1898. The affiliated societies, nine in all, are: the Anthropological, Biological, Chemical, Entomological, Geological, and Philosophical Societies of Washington, the National Geographic Society, the Columbian Historical Society, and the Medical Society of the District of Columbia. The President is J. R. Eastman. A plan for the selection and printing of papers has been carefully matured, and accepted papers will soon go to press. It is intended to conduct courses of lectures, and to hold occasional meetings for the reception of new scientific material. It is probable that one of the chief functions of the Academy will be to act as an advisory body to the United States Government on questions involving scientific knowledge.

*Science Work* for April reproduces in good style an excellent photograph of the Scuir of Eigg, taken from E.S.E. by Mr. A. S. Reid, who has taken a complete series of views illustrating the geology of the island, for the Geologist Photographs Committee of the British Association. This journal now bears the imprint of Messrs. Dawbarn and Ward, the publishers of the *Photogram*.

An interesting comparison of the eyesight of Europeans and "natives" (Kaffirs, Hottentots, Basutos, etc.) has been made by Dr. Beheim, and is published

in the *African Review*. Out of a total of 1853 natives, 1509 had a vision equal to the normal vision of Europeans, 257 were above, and 87 below the average. This bears out what was indicated by Mr. Hudson in one of his interesting essays.

In the *Scientific American* for April 15, there is a note by Mr. Charles H. Coe, to the effect that the honey-bee was probably introduced by the Spanish settlers in Florida, for Bartram, who explored the country in 1773, mentions honey and beeswax as articles of barter among the Indians.

In the May number of our ably-edited contemporary *The Veterinarian*, which is probably less known to biologists than it should be, we find a note to the effect that Baron de Parana, of Porto do Cunha, Rio de Janeiro, has now succeeded in getting five zebra hybrids between a true Burchell's zebra and mares.

# OBITUARIES.

## JOSEPH WOLF.

Born 1820; Died April 20, 1899.

We regret to record the death of Joseph Wolf, the artist and painter of animals, who has been for so many years prominently before the zoologist and the general public. Wolf was born near Coblentz, was early apprenticed to a lithographer in Antwerp, and was "discovered" by Hermann Schlegel. He came to England in 1848, and was soon accepted by painters of all ranks. His studio became a rendezvous, and he numbered among his friends everyone worth knowing among the explorers, zoologists, and artists. Wolf's work is best known through his illustrations to books, and of these Gray's "Genera of Birds," Gould's "Birds of Great Britain," and his own "Life and Habits of Wild Animals" are well known. A very fine series of his drawings is to be found in the apartments of the Zoological Society of London, and many of the plates in the *Proceedings* of this Society and of the Linnean Society, between 1850 and 1870, are by him. He was engaged only just before his death in correcting the proofs of an article by Mr. Trevor-Battye upon his work for the *Artist*. His life, by Mr. Palmer, appeared a short time ago.

## SYLVANUS CHARLES THORP HANLEY.

Born January 7, 1819; Died April 5, 1899.

This well-known conchologist of the older school began to publish in 1840, his first work being "The Conchologist's Book of Species." This was followed rapidly by "A Catalogue of Recent Bivalve Shells," English edition of Lamarck's "Species of Shells," 1843; "Index Testaceologicus," 1856, with W. Wood; "Ipsa Linnaei Conchylia," 1855; and "Conchological Miscellany," 1854-58. He brought out the second edition of Swainson's "Exotic Conchology," combined with Edward Forbes on a "History of British Mollusca," and was the author of many minor papers. He was a B.A. of Oxford (Wadham College), lived for many years in Hanley Road, Hornsey Rise, London, and died at Penzance.

## JABEZ HOGG.

Born April 4, 1817; Died April 23, 1899.

Few names are more familiar in microscopy than that of Jabez Hogg. Born at Chatham, he was educated at Rochester Grammar School, and in 1843 joined the staff of the *Illustrated London News*. In 1845 he entered as a student at Charing Cross Hospital, becoming M.R.C.S. in 1850, and taking up practice in ophthalmology; was at the time of his death consulting surgeon to the Royal Westminster Ophthalmic Hospital. He was a prominent Freemason, receiving

from the Prince of Wales in 1889 the office of Grand Sword-Bearer in Supreme Grand Chapter. Hogg was best known to zoologists for his "The Microscope," of which the first edition appeared in 1854, and the fifteenth in 1898. Among his other works are "Manual of Photography," 1845; "English Forests and Forest Trees," 1853; "Colour Blindness," 1863. He was also the author of numerous papers on the eye, and conducted an investigation into arsenical wall-papers, the results of which he published. He died at Palace Gardens Terrace, London.

## GEORGE CHARLES WALLICH.

BORN NOVEMBER 16, 1815; DIED MARCH 31, 1899.

WALLICH was born at Calcutta. His father, Nathaniel, was well known as an Indian botanist. George was educated at Edinburgh University, became M.D. 1836. He entered the Indian army in 1838, and served in the following campaigns: Sutlej, 1842; Punjab, 1847; and Southal, 1855. He retired in 1859.

The author of numerous papers, Wallich's especial claim to remembrance is his "North-Atlantic Sea Bed," 1862, in which was proved conclusively the existence of organic life at great depths in the ocean, depths which since that time have been far surpassed. He contributed some twenty papers to the literature of the Protozoa, establishing the remarkable genus *Rupertia*, and he was the first to throw suspicion on the organic nature of "Bathybius."

## HEINRICH KIEPERT, 1818-99.

HEINRICH KIEPERT, whose death was announced at Berlin on April 22, was one of the most distinguished geographers of his time. Born in Berlin in 1818, he was a member of that University, to which he returned in 1859 as Professor of Geography, after a few years of labour in the Geographical Institute of Weimar. His especial subject was South-Eastern Europe and Western Asia, of which parts his maps were authoritative and masterly; at the same time he devoted much attention to the ancient world, and contributed largely to the elucidation of its cartography. His Asia Minor was perhaps his masterpiece.

## FRANZ VON HAUER.

BORN JANUARY 30, 1822; DIED MARCH 21, 1899.

THE director of the Vienna Museum was born in Vienna, and was the son of Joseph v. Hauer, Vice-President of the Chamber. He was educated in Vienna, and obtained his technical knowledge of geology in the Berg-Akademie of Schemnitz. In 1843 he spent some time at the ironworks of Steiermark, and in 1844 was called to the Haidinger Museum in Vienna. Becoming assistant to Haidinger in 1846, he published his first paper *Die Cephalopoden des Salzkammergutes*. He became attached to the K. K. Geologischen Reichsanstalt in 1849, and was employed surveying in all parts of the Monarchy till 1867, when, on the retirement of Haidinger, v. Hauer became director of the Reichsanstalt. In 1886 he was appointed superintendent of the Natural History Museum in Vienna. Von Hauer published many papers, and was responsible for an important geological map of the Austro-Hungarian Monarchy in twelve sheets (Wien, 1867-73), the fourth edition of which appeared in 1884.

## SIR FREDERICK M'COY, K.C.M.G., F.R.S.

Born at Dublin, 1823; Died at Melbourne, 1899.

Frederick M'Coy was educated for the medical profession at Dublin and Cambridge, but early devoted himself to natural science. Sir R. Griffith invited him to make the palaeontological investigations for the geological map of Ireland for the boundary survey, the results of which he published in 1844. Afterwards he joined the Geological Survey of Ireland. In 1854 he was appointed the first Professor of Natural Science in Melbourne University. His services to Victoria were great, notably in regard to the geological survey of the colony, as a member of various commissions, and as the founder of the Melbourne National Museum. In 1880 he was elected F.R.S., and was one of the first to receive the honorary degree of D.Sc. from Cambridge. In 1886 he was made a C.M.G., and in 1891 he was promoted to be K.C.M.G. Sir F. M'Coy also received the Order of the Crown of Italy from King Victor Emanuel, the Emperor of Austria's gold medal for arts and sciences, the Murchison medal of the Geological Society of London, and many similar distinctions.

The deaths are also announced of Mariano de la Bárcena, director of the Central Meteorological Observatory, Mexico; on May 1, in his 75th year, Dr. Ludwig Büchner, of Darmstadt, the famous author of "Kraft und Stoff" (1855), and many books and papers on evolutionist subjects; Dr. C. Brongniart, Paris, well known for his entomological work, especially on palaeozoic insects; on April 1, at Halifax, Henry Thomas Soppitt, fungologist and field-naturalist, in his 41st year; Adolf Walter, a well-known ornithologist, at Kassel, on February 4, in his 82nd year.

# NEWS.

The fifteen candidates selected by the Council of the Royal Society to be recommended for election into the Society this year are :—Prof. W. F. Barrett ; Mr. Charles Booth ; Surgeon-Major David Bruce ; Mr. H. J. H. Fenton, M.A. ; Mr. J. S. Gamble, M.A. ; Prof. A. C. Haddon ; Dr. Henry Head ; Principal C. Lloyd Morgan ; Mr. Clement Reid ; Mr. H. S. H. Shaw ; Dr. E. H. Starling ; Prof. H. W. L. Tanner ; Mr. R. Threlfall ; Mr. A. E. Tutton ; and Prof. B. C. A. Windle. As will be seen, natural science is well represented, and *Natural Science* also.

Among the five new members of the U.S. National Academy of Sciences are C. E. Beecher, the palaeontologist, and E. B. Wilson, the embryologist.

On April 27, at Cambridge, Prof. Sir William Turner and the Rev. Prof. Thomas Wiltshire, were presented for the honorary degree of Doctor in Science.

The M'Gill University of Montreal has conferred the degree of LL.D. on Mr. Rudyard Kipling.

The gold medal of the American Geographical Society has been awarded to Sir John Murray, K.C.B.

Both the gold medals of the Royal Geographical Society have this year been awarded to Frenchmen. The recipients are Captain Binger and Mr. Foureau. The former is well known for his work in West Africa, particularly his exploring of the Kong country and of the vast and important area within the great bend of the Niger. The other medallist, Mr. Foureau, is at present engaged on a mission in the Sahara as one of the leaders of the Foureau-Lamy expedition. He has done more, perhaps, than any other man to add to our knowledge of the Sahara. The minor awards are four in number, and of these one goes to the Swedish naturalist, Dr. Nathorst, for his exploration of the Spitzbergen Islands and the seas between Spitzbergen and Greenland ; another to the Hon. David Carnegie for his journey across the great interior desert of Western Australia, described in his recently published book, *Spinifex and Sand ;* a third to Mr. Albert Armitage for his scientific work with the Jackson-Harmsworth Arctic Expedition ; and the fourth to Captain Sykes for three journeys in Persia, during which he made important corrections and additions to the map of that country, and has done much to clear up the geography of Marco Polo.

Albert Lindström has been appointed honorary professor of anatomy at Stockholm.

The fourth Annual Congress of the South-Eastern Union of Scientific Societies was held at Rochester from May 25 to 27, 1899, under the presidency of Professor G. S. Boulger.

The Ealing Natural Science and Microscopical Society are making a collection of lantern slides to promote the study of natural science. They hope to obtain

such slides as shall illustrate definite subjects, such as the "Structure and Life-History of Plants and Animals," "Vulcanology," "Geology," "Astronomy," "Physics," "Local Phenomena of Scientific Interest," etc. The loan of negatives to make slides from will be of considerable value. Mr. B. B. Woodward, 120 The Grove, Ealing, W., will be glad to hear from any one interested.

At a meeting of the Geologists' Association on May 5, a paper on "The Drainage of Cuestas," by Professor W. M. Davis, of Harvard, was communicated. The paper dealt with the unsymmetrical longitudinal uplands (such as the Oolites and Chalk give rise to in this country) characterising certain denuded coastal plains, to which the author proposes to apply the distinctive term "cuestas." He discussed (1) the development of these topographical features, (2) the evolution of their drainage, and (3) the significance in this relation of certain details of valley form, especially as exhibited in the Swabian Alp of Würtemburg and the Cotswold Hills.

At the annual session of the U.S. National Academy of Sciences, which opened in Washington on April 18, communications were submitted on the Tawner deep sea tow net, by Alexander Agassiz; on the Acalephae of the East Coast of the United States, by Alexander Agassiz and A. G. Mayer; on "Palolo," by W. M'M. Woodworth; on *Ophiura brevispina*, by W. K. Brooks and C. Grave; and many others.

The Canvassing Committee of the Birmingham University has informed Mr. Chamberlain that the conditions attached to the gifts of Mr. Carnegie and the anonymous donor had been fulfilled, the subscriptions having reached £254,580. The anonymous donor, recognising that an endowment of a quarter of a million, although sufficient for a start, must soon be largely augmented, has also offered to contribute the last £12,500 if the fund were raised to £300,000 by the end of June. It was decided to make an appeal to the people of the city and district for the amount necessary to secure this additional offer.

*Science* announces the following gifts:—Mr. W. K. Vanderbilt has given 100,000 dollars to Vanderbilt University to build a new dormitory; and Miss C. W. Bruce has given 10,000 dollars to Columbia University for the astronomical department.

Applications for the Jodrell Professorship of Zoology in University College, London, vacant by the appointment of Prof. Weldon to the chair in Oxford, are invited by June 5.

Among the subjects of lecture and discussion at the annual meeting of the German Zoological Society convened in May at Hamburg, we notice:—The Hamburg Museum (Prof. K. Kraepelin); the scope of natural selection (Prof. L. Plate); Atolls and their Plankton (Dr. A. Krämer); Arctic and Antarctic faunas (Dr. G. Pfeffer); the statistical study of variation (Dr. G. Duncker); the development of *Bdellostoma* (Dr. F. Doflein); all of them most interesting subjects.

The *American Naturalist* notes that nearly forty applications were received for a special four weeks' course of instruction on poultry culture, held in January of this year at the R. I. College of Agriculture and Mechanical Arts (Kingston).

The biological laboratory of the Brooklyn Institute of Arts and Sciences at Cold Spring Harbour, Long Island, will be open for its tenth session during July and August 1899. Courses are offered by Dr. C. B. Davenport, Professor H. S. Pratt, Professor C. P. Sigerfoos, Dr. D. S. Johnson, Professor F. O. Grover, Mr. N. F. Davis, and Mrs. G. C. Davenport. Opportunities will be afforded for original work, especially on variation. The laboratory is equipped with a naphtha launch, bacteriological apparatus, and a good working library.

Boarding accommodation is provided. Applications may be made to Professor F. W. Hooper, 502 Fulton Street, Brooklyn, N.Y., or to Dr. C. B. Davenport, the director of the laboratory, Harvard University, Cambridge, Mass.

In connection with the housing of London University at the Imperial Institute, it is reported that the Government will arrange to take over the titles of the building, so that the University will be tenants of the Exchequer and not lodgers of the Institute.

The colonies of the West Coast of Africa have adopted a scheme whereby picked boys shall be sent for training in horticulture and economic agriculture, first to Jamaica and afterwards to Kew Gardens. This scheme is due to the success that attended the experiment initiated by the then Governor, Sir A. Moloney, of sending in 1890 two native lads from Lagos to be trained as gardeners at the Jamaica and Kew Gardens. After spending two years and a half in Jamaica, they were attached to Kew Gardens for some months, and are now in charge of branch botanic stations in Lagos, growing and preparing agricultural products, and giving instruction.

The *Scientific American* records the wreck in Magdalena Bay of the schooner "Ella Erland," and the consequent unfortunate end of a small expedition under Professor A. W. Anthony. But the naturalists on board, who had intended to study the fauna and flora of the equatorial Pacific coast, escaped with their lives at least, and have returned in safety.

Two new buildings in the new Zoological Park of New York are rapidly nearing completion. These are the reptile house and the winter house for birds. Much work has also been accomplished upon the various outdoor animal dens. In a hollow in the park is a body of water which will be utilised for aquatic rodents. The beaver pond is also ready, while up on the elevated portions among the rocks, work on the bear dens is well under way.

On the occasion of Spallanzani's centenary, celebrated recently at Scandiano, a laboratory was inaugurated to commemorate and continue his work. Professor Luciani of Rome and others delivered commemorative addresses.

We are pleased to learn that *Science Gossip* is to be enlarged. This is the result of the success with which Mr. Carrington has carried out the editorial work. Among other changes we note the appointment of Miss Flora Winstone as assistant editor. Miss Winstone for several years past has assisted largely with the editorial work, and in the absence of the editor abroad has on several occasions taken sole editorial charge of the magazine.

We have heard that there are adequate physiological reasons to account for the absence of blue roses, and we have not pined for them. It is not, therefore, with either faith or hope that we read the report of the Vienna correspondent of the *Standard* that an azure-blue rose has been produced in Kezaulik, the Valley of Roses, in Bulgaria.

The results of the Geodetic survey of the United States, which has been in progress for over fifty years, are to be published immediately. Measurements, such as the total distance across the Continent (2625 miles) will be known so exactly that the error will be less than 100 feet. In making this survey many stations have been used at altitudes of 10,000 to 14,000 feet, necessitating the application of new formulae.

A new Arctic expedition will soon sail from St. John's, Newfoundland, under the charge of A. Barclay Walker, an English yachtsman. Mr Walker intends to cruise in Arctic waters in the "Dundee," a steam whaler, during the next six months, with a party, including representatives of the Smithsonian Institution. They will probably attempt to reach the headquarters of Lieut. Peary, in Robeson Channel.

On May 8, Sir Martin Conway lectured to the Royal Geographical Society on his latest piece of exploring work in the Bolivian Andes. His survey was mostly confined to the western slopes of the Cordillera, "the backbone of Bolivia," and he hopes on his next visit to make an equally careful survey of the eastern slope. He gave some striking illustrations of the power of rivers and glaciers to eat their way backwards through vast mountains—in one instance a tributary of the Beni River, which flows into the Amazon and so into the Atlantic, having actually eaten its way back, clean through the Cordillera, and so drained a portion of the western slope of the range into the Atlantic. Sir Martin stated that the whole range was highly mineralised, but of the distribution of the minerals he could not say much. Gold was found at several points, chiefly on the east side of the range. Just below the snowy mass of Cacaaca on the west there is a really enormous vein of tin, while antimony, cobalt, and, he believed, platinum had been found in different parts. Subsequently Sir Martin Conway climbed Aconcagua, but his ascent was not a scientific, merely a sporting expedition.

The *Scientific American* gives a record of slaughter which, if authentic, is not without interest. It is to the effect that on Prince Schwarzenberg's preserves in Bohemia 106,604 wild animals were killed during last season,—including 200 deer, 250 boars, 27,000 hares, 39,000 grouse, and 6000 wild geese and ducks.

An edge of the seamy side of the collecting mania was recently exposed in Paris by the detection of an "egg-forger," who "made fly-catchers' eggs into those of the silk tail, and ducks' eggs into those of falcons, and larks' eggs into nightingales." A penguin's egg was made—not exactly out of nothing, but out of plaster of Paris and the like. As the counterfeits are almost indistinguishable, they would serve most purposes very well, and one almost wishes prosperity to the crime.

In a recent number of *La Nature*, Mr. Henry Castrey gives an analysis of the manna of the desert (*Canona esculenta*), which appears abundantly on the sand after rain, sometimes in great heaps. The analysis shows the following composition:—Water, 16 per cent.; nitrogenous matter, 14 per cent.; non-nitrogenous matter, 29 per cent.; carbohydrates, 32 per cent.; fat, 4 per cent.; mineral matter, 5 per cent.

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